Essays in Impact Evaluation for Development

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CHAPTER I

Introduction

This dissertation assesses four distinct development interventions. The first intervention presented in Chapter II aims at improving the capacity of rural Filipinos to make informed decisions about insurance. In a set of randomly selected communities an awareness campaign was conducted and we assess the effects of this campaign on insurance comprehension, perception towards and demand for insurance. We find no evidence that the intervention improved knowledge in the short term but we detect positive effects on the attitude towards insurance and on the awareness about issues related to insurance in the campaign villages. Respondents who live in campaign villages but did not receive a brochure have significantly lower scores on the knowledge test questions. This is in line with the hypothesis that issues of insurance are complicated to grasp for low-income individuals and relying on social networks to transmit insurance knowledge is prone to fail. It might create adverse affects. The second chapter analyzes two interventions that aim at strengthening producer organizations of farmers in Uganda. We find some evidence that farmers adapt their labor input in aspiration of improved market access through their producer organization. The farmers unleash formerly untapped household labor resources in anticipation of improved transparency of the sales processes of their organization. In anticipation of

relieved financial constraints at harvest time, the farmers increase their demand for hired labor in crop production. Heterogeneous effects show that trust in the leaders of the producer organization plays a decisive role for the interventions' impact on labor decisions. Contrary, to the initial hypothesis we cannot find evidence for a positive impact of the intervention on trust in the producer organization or its leaders. Chapter IV and V both report the effect of formalization interventions conducted by microfinance institutions. The interventions try to improve the knowledge of informal entrepreneurs on the benefits of registering their income activities. We hypothesize that formalization can benefit a distinct segment of informal entrepreneurs. The first formalization intervention is implemented in India and succeeds in bringing large parts of the treatment group to register their activities with official authorities. The second intervention is implemented in Burkina Faso and fails to bring about substantial change in the entrepreneurs' official status. In Burkina we observe a small positive impact on the degree of formality but cannot document an increase in formal registrations. For neither of the two interventions we observe economically significant effects on second order outcomes that would support the hypothesis that formality is advantageous. The large impact on the first order outcome, formal status, indicates that such advantages must exist in India but are unobservable to us. In Burkina, the intervention shows an initial positive effect that does not persist over the period of investigation but rather fades off. This suggests that the formal business environment is not sufficiently attractive for the microfinance clients.

CHAPTER II

The Impact of Insurance Literacy Education on Knowledge, Attitude and Behavior - A Randomized Controlled Trial

2.1 Introduction

Microinsurance is a promising tool to reduce the vulnerability of low-income households in developing countries. However, microinsurance schemes tend to suffer from low take-up (Dercon et al., 2008; Ito and Kono, 2010). Further, low usage and retention in public and private schemes alike reflect the lack of acceptance of these products among the target population. In part, demand side problems like lack of understanding and lack of familiarity with the concept of insurance contribute to the low demand (Cole et al., 2013; Giné et al., 2008)[1]. Financial education, in theory, directly affects an individual's financial comprehension and thereby aims at improving the capability to make informed financial decisions. Evidence from developed countries documents a strong positive link between financial literacy and financial decision making (Duflo and Saez, 2003; Lusardi, 2012; Lusardi and Mitchell, 2011; Lusardi and Tufano, 2009; Sekita, 2011; van Rooij et al., 2011).

[1]Other possible reasons for low take-up are lack of trust in the insurer low quality of microinsurance product or barriers that hinder the individual from buying, e.g. high direct or indirect costs. The focus of the present study is solely on understanding and awareness of microinsurance

Despite the recently increased interest in financial literacy, the interventions that try to improve it in developing countries remain relatively rare and the empirical evidence that exists does not present a clear picture on the effectiveness of such endeavors (Holzmann, 2010). This holds also for the evidence on insurance literacy interventions. Here the existing knowledge focuses on distinct groups, e.g. occupational groups, to promote specific insurance products. To our knowledge this is the first rigorous evaluation of an insurance education campaign that targets the general rural population in a developing country.

In developing countries, plain misconception or/and adverse attitude add to the problems that prevent individuals from buying microinsurance products. Insurance literacy education could be a remedy. This study presents evidence from a randomized controlled trial that assesses whether distributing insurance education material can affect knowledge and attitude in a low-income population and whether this ultimately provokes changes in the demand for a health insurance product.

For the intervention an educative brochure was developed and distributed in a randomly chosen set of villages. Within the treatment villages, henceforth also called campaign villages, not all households received a brochure due to restricted funds. Distribution was carried out in a campaign like manner, whereby the distributors had instructions to skip households following a distribution key. Among the households that did not receive a brochure some were later allocated to the campaign village control group. Comparing this first control group to a second one drawn from non-campaign villages allows estimation of a spill-over effect within the campaign villages.

We evaluate the effect of insurance literacy education on several outcomes. First, we compare knowledge outcomes for treated and controls. We estimate the brochure's direct effect and the spillover effect on control households and find that the campaign

fails to improve short-term knowledge of brochure recipients. Instead, we document significant negative effects on the control group in campaign villages. This suggests that the assessment of unintended negative spillover effects is essential as they might distort the intended effects. Further, this finding creates doubt regarding the use of social networks to promote financial litercy.

Secondly, we estimate the impact on attitude towards insurance. We observe significant effects on brochure recipients and non-recipients from campaign villages whereby the direct effect and the spill-over effect point in the same direction. Despite the intervention's success to provoke attitudinal change, we cannot document any significant impact on our third outcome measure, take-up of a micro health insurance product. We assess this outcome by matching our survey data with administrative data from the national health insurer, PhilHealth. This allows us to estimate the campaign's impact on the individual level. Further, we analyze the membership information on the aggregate village level for the six months following the intervention. On none of the two levels can we document any impact on PhilHealth registrations.

The effects on the different dimensions of attitude suggest, that the campaign did not only increase awareness about the usefulness of insurance, it also created awareness about the financial liability that an insurance contract brings about. Campaign village respondents are more inclined to state that insurance is something expensive. Further, an indicator measuring the perception of accessibility of insurance was affected negatively. In part, this indicates that other obstacles to take-up exist besides perception.

The structure of this paper is as follows. The next section discusses the existing literature on financial education in general and for microinsurance in particular. It puts our contribution in relation to the existing stock of knowledge. The third

section presents the design and the implementation of the randomized controlled trial. Section four presents the sample and section five the experimental results. Section six concludes.

2.2 Motivation

Microinsurance like microcredit serves low income populations that are excluded from traditional commercial financial products due to their socio-economic situation, characterized by low irregular incomes, informal activities, low eduction, etc. Yet, the case of microinsurance in terms of understanding and perception is a different one from the case of microcredit. Microcredit builds on existing informal schemes, i.e. rotating savings and credit associations (ROSCAs) for group lending schemes or moneylenders for individual lending. As a result, the target population is familiar with the mechanisms that microcredit providers use. Traditional or informal insurance mechanisms are based on reciprocity. Everybody contributing to the scheme will benefit from it at some point in time (Churchill, 2002). A common example, that exists also in the Philippines, are funeral groups which are informal associations providing for the costs occurring from the death of a community member.

Microinsurance, like standard insurance, is based on the concept of risk pooling where regular premium payments of all insured individuals cover the financial losses of those suffering from an insured event. Those who are insured but remain unaffected might never receive compensation nor reimbursement of the premium payments they made during the contract. The target population's lack of familiarity with this mechanism creates room for misunderstanding and misconception. In addition, low education poses an obstacle to understanding terminology, contract details

and procedures, thereby resulting in further discouragement to acquire an insurance product.

Insurance literacy education is a potential way to remedy low familiarity, understanding and acceptance of microinsurance products. Hence, it might lead to increased voluntary take-up of such products. The underlying causal chain is the following: Increased education about insurance improves understanding and alters, either subsequently or simultaneously, the attitude towards such products. These changes improve the capacity to make informed decisions which might then translate into increased take-up. The present work analyzes the entire causal chain. We first look at whether knowledge and attitude can be affected and, secondly, study whether the demand for a micro health insurance product changes as a result of the educational campaign. Of course, this causal chain relies on underlying assumption, e.g. no other barriers besides insurance knowledge and perception must exist. Further, for the last link to hold it is neceassary that the available microinsurance product can be considered a viable mean to protect households from financial risk through health shocks.

For developing countries little evidence on the importance of financial literacy exists (Holzmann, 2010). Cole et al. (2011) find high correlation between financial literacy and the demand for financial services in Indonesia and India. This suggests that also in a developing country context better understanding of financial products could lead to increased demand.

Recently, several studies tried to assess whether education is an effective remedy to low take-up of microfinance products. The results as well as the approaches are mixed. Cole et al. (2011) study the causal effect of a financial education module on the demand for financial products. Their results are insignificant for the general

population but they report modest positive effects on take-up of savings accounts in a sub-sample of individuals with low education.

Cai et al. (2011) evaluate the effect of insurance education on take-up of a rice production insurance in China. They report significant positive effects on take-up and a slightly lower but still significant spill-over effect on non-participants through social interaction. Three particular features of the intervention they study need to be mentioned. The insurance was offered to the participants immediately after the training, solving problems of accessibility. The insurance premium was deducted from an agricultural subsidy that all study subjects were eligible for, which solves the problem of liquidity constraints. The study focused on a distinct group of rice farmers and the training contained explanations of the specific product that was offered. Cole et al. (2013) also look at take-up of an insurance product among farmers, but they report contrary results as their education module had no effect on take-up.

Carpena et al. (2011) focus on the link between education and understanding. They find that education in itself does not improve understanding immediately and that attitude towards financial products can be affected more easily. In that respect their results are in line with our findings.

When it comes to financial (and insurance) literacy campaigns one needs to differentiate general and product-specific education. On the one hand, general literacy education and awareness campaigns address the target population's unfamiliarity with the concept of insurance and explain terminology, different insurance types and benefits. Such campaigns do not promote specific insurers or their products. As a result of diverse issues (e.g. free riding, low perceived value of literacy education, mistrust in insurers, etc.) it is mostly the government or non-profit institutions that

carry out such large scale interventions[2]. This type of intervention has a social rather than a commercial character. On the other hand, product-specific insurance education is closely related to traditional commercial marketing and mostly conducted by insurance sales personnel. Such campaigns might also cover general insurance terminology but ultimately aim at explaining and selling specific products. This kind of customer education explains contractual features (e.g. premium payment or claim procedures) and is important not only for increasing sales but also to prevent customer dissatisfaction leading to low renewal rates. The commercial purpose of such interventions is prevalent and most likely obvious to the target population. The educator might even be motivated by commissions. Therefore, the effectiveness of commercial insurance education on demand is subject also to the target population's trust or mistrust. One specific characteristic of the present study is that the educational campaign neither explains a specific product nor does it focus on a specific occupational group.

In the analysis of financial education activities the intensity of the treatment is also an important dimension. Intensity in the above mentioned studies varies from weekly meetings over the period of several months to one-day training sessions of several hours. We study an intervention that increased interest in issues of insurance at the day of brochure distribution and, through the brochure, made information available for a much longer period of time.

[2]In South Africa, for example, SAIA (South Africa Insurance Association) a conglomerate of commercial insurers collects funds from all members to improve the general public's insurance understanding

2.3 Experimental Design

In Fall 2010, we conducted the educational campaign in the region of Caraga in the Philippines. Caraga is a mostly rural region on the island of Mindanao. In 2009, it had a poverty incidence of 45.9 percent making it the poorest region in the country[3]. We selected 60 villages (barangay) that were easily accessible from the provincial capital Butuan and that had a population of 800 to 2500 inhabitants. All barangay are located close to the national highway in the provinces Agusan del Norte and Agusan del Sur. Out of these, 34 were randomly chosen as campaign villages, i.e. treatment villages. In the remaining 26 villages we interviewed control respondents. In the campaign villages we distributed a brochure in a door-to-door manner similarly to an awareness campaign. The distributors were allocated to different parts of the village in teams of two. They had instructions to choose the recipients at their own merit and to not provide direct neighbors with the brochure.

Only households where the head or the head's spouse were available received the brochure. Before handing over the brochure, the recipient answered a short questionnaire with questions on insurance literacy and attitude which gives us an idea of the initial knowledge and was supposed to raise interest of the respondent in issues of insurance. The enumerator prepared a sketch of the household's location so that we would be able to relocate it for the follow-up interview. On average, 60 brochures were handed out per barangay and it was not announced that we would visit again for an extensive follow-up survey one week later. While the short questionnaire gives us an idea on the level of understanding and attitude in the treatment group, it cannot serve as a baseline since we did not conduct this "delivery survey" with the control group.

[3]See `http://www.nscb.gov.ph/poverty/2009/tables_basic.asp`.

A week after distribution we revisited the campaign villages, conducted the follow-up survey with the recipient households and selected the control group among the non-recipient households. This group of respondents, henceforth called control group I, was likely to be affected by the campaign via social interaction, i.e. spill-over. In addition, we surveyed a second control group in control villages, henceforth called control group II. Surveying individuals from villages where no brochure was distributed precludes contamination of the control group which would otherwise bias our impact estimates. It also allows us to quantify the spill-over effects on knowledge and attitude that occurred within the campaign villages by comparing control group I - *with spill-over effects* - to the unaffected controls.

The brochure was initially developed by the German International Cooperation (GIZ) in Ghana. We adopted it to the setting of the Philippines. Its purpose is to raise awareness and understanding of the benefits and processes of insurance. Using a comic and simple text in the local language it explains the main processes of insurance using the example of a health insurance. It also contains explanations on insurance terminology and other types of insurances (e.g. life insurance, etc.).

Table 2.1 reports the total numbers of brochure recipients and respondents. In total we distributed 2243 brochures. Only 1563 recipient households were revisited for the follow-up interview. From the outset we decided to distribute more brochures than the number of intended follow-up interviews. In this way, we wanted to prevent that attrition creates a reduction of the final sample size. A total of 680 brochure recipients were not selected for the follow-up visit and dropped out.

The design of the brochure's cover page is the second treatment. It features four messages each emphasizing a different motive for insurance take-up. We randomized these messages on the household level and tested for sensitivity of knowledge

Table 2.1: Number of distributed brochures and interviews

	Control barangay	Campaign barangay
Control households (only follow-up)	461	609
Brochure recipients (only before)		680
Brochure recipients (before and follow-up)		1,563
Total	461	2,852

and attitude. The four different messages featured solidarity, optimism, trust and protection (See the design of the brochure in Figures 2.2 - 2.4 in the Appendix). We also included one design that has a blank cover and which does not allude to any message. We cannot find any significant effects and therefore the results on the messages are not reported in the following.

2.4 Socio-Economic and Demographic Summary Statistics

Table 2.2 displays summary statistics according to treatment status. Columns 1 to 4 present averages for all respondents residing in campaign villages (T=1, i.e. brochure and control I respondents), for brochure respondents only (TT=1, i.e. village was treated and household was treated), for respondents from campaign villages who did not receive a brochure (TT=0, i.e. village was treated but not household), and for control respondents from control villages (T=0, i.e. neither village nor household were treated), respectively. In the last two columns we present p-values of t-tests that compare the mean of brochure recipients to the respective control group.

The sample consists of households engaging in low income formal and informal activities. On average the household head is about 42 years old, has around eight years of education and is male in 90 percent of the cases. The most prevalent income sources are farming and service provision. Almost 50 percent of the households claim to have some income from self-employed activities.

Access to commercial financial products is low in general. Only about 10 percent of the sample state that they have a bank account. As for insurance coverage, we observe that the share of PhilHealth members is high at 73 percent of which approximately half are paying members. The paying members are either enrolled through their employment, if the household has any income from formal employment, or voluntary members. Those who are not paying members are enrolled in PhilHealth's Sponsored Program which is offered free of charge for indigent households. These results vary highly from what we observed in a study conducted in the same region one year earlier in September 2009. At that time we found that merely 15 percent of the households were covered by PhilHealth. From anecdotal evidence we learned that in preparation of the super election year 2010, when elections were held on every administrative level in the Philippines, temporary PhilHealth memberships were distributed by candidates. The high level of enrollment sharply reduces our chances of measuring an effect on take-up.

Regarding the balancing by treatment group, we observe some variables that are significantly different in means between the recipient and the control group. Within the campaign villages, we find that the share of mobile phone owners is significantly higher for brochure recipients compared to controls. We observe that contact to barangay officials is significantly higher at the 10 percent significance level. Partly, the difference in this self reported measure might result directly from the intervention. After our first visit in the village, barangay officials might have visited brochure recipient households to inquire about the visit of our brochure distributors, especially so because the elections for the post of the barangay captain were coming up when we conducted the campaign. Additional evidence for this assumption is provided by comparing recipients and control group II. In control group II the share

of respondents who have contact to officials is 7 percent lower and the mean difference is statistically significant at the 1 percent level.

Without explanation remains the difference in shock history between recipients and control group I. Among the recipients 34 percent report a death or a hospitalization in the last three months (*Shock history*=1) compared to only 26 percent in the control group I which is highly significant. Between control group II and the treatment group we do not observe a statistically significant difference. One might argue that control group I underreports such health events which might result directly from the treatment.

Comparing recipients and control group II reveals a priori differences for a few variables: Fewer female heads, higher schooling of the head and higher number of children. Enrollment in PhilHealth, access to bank accounts, land ownership and reported shocks are all balanced. The parametric analysis allows us to control for the imbalances.

2.4.1 Non-PhilHealth Members

Since our interest is on the campaign's effect on take-up of insurance we also display the summary statistics for respondents who are not yet enrolled with PhilHealth (Table 2.3). One striking difference between the full sample and the non-PhilHealth sample is the higher share of female household heads among the latter, i.e. the uninsured. This indicates a correlation between the gender of the household head and insurance membership. More formal employment among men might offer one explanation for this difference. PhilHealth is mandatory for formally employed. In the case were formal employment is higher among men, female household heads might

be insured less often. We rather observe the reversed situation in non-campaign villages. Here, the small sample size calls for caution.

As to the balancing, we only observe two variables that differ significantly between the treatment group and the respective control group. These are *Female head* which is up to 15 percent lower and *Mobile phone*. For the latter, we observe approximately 60 percent of the treatment group and control group II who own a mobile phone while this share is only at 45 percent in control group I. With the exception of gender of the household head, the main demographic variables, i.e. age, schooling and number of children are balanced in the non-PhilHealth sample.

2.4.2 Female Respondents

The majority of respondents is female. This is not a result of the sampling strategy but rather related to the survey implementation. We conducted our interviews during the morning and the early afternoon. At this time of the day only few male household heads are available. Table 2.3 presents summary statistics for the female sub-sample, i.e. the sample excluding all male respondents. The results are fairly similar to those based on the entire sample.

2.5 Experimental Results

This section presents the impact results of the insurance literacy campaign. All estimated effects reflect Intention-to-Treat (ITT) effects since we observe whether the household received a brochure but not whether the respondent really took time to study it. Each of the three following subsections presents mean comparisons of the outcome indicators and estimation results.

The first subsection looks at the impact on insurance literacy. We construct indices from a battery of knowledge questions that assess the level of insurance

comprehension. The second subsection looks at the effect on attitude. Here we apply explorative multivariate analysis to construct factors from questionnaire items that capture the respondent's attitude towards insurance.

The third subsection addresses the impact on behavior. We describe the microinsurance product that PhilHealth offers and use administrative data to assess the effect on new membership registrations. We study a period of six months following the education intervention. Again we present mean comparisons as well as ITT estimations. We complement the individual level analysis by looking at the campaign's effect on the barangay level.

2.5.1 The Impact on Knowledge

We assess the respondent's comprehension of procedures, terminology and benefits of insurance. At the day of the campaign (before handing over the brochure) we asked the recipients three knowledge and three attitude questions. These questions are a subset of the questions included in the follow-up survey[4]. They serve two purposes. First, we are able to test whether individuals selected to receive a brochure and selected to respond to the follow-up survey differ from those who were only selected to receive the brochure but were not interviewed at follow-up. This is important to ascertain that no selection occurred at this stage. Second, we can compare the initial knowledge and attitude of our treated sample to the control II sample.

Initial Knowledge

Table 2.5 presents the average scores for the knowledge test questions at the day of distribution. Column 1 lists the average of correct responses from all brochure

[4]See the knowledge test questionnaire items in Figure 2.8 in the Appendix.

recipients. In Column 2 we find the average for the group of respondents interviewed at delivery and at follow-up. Column 3 lists the results for respondents interviewed only at delivery and Column 4 presents the averages for control group II.

We observe that control group II has significantly higher average results for two out of three questions. For the first question we find a small difference of 4 percent that is significant at the 10 percent level. For the third question however 75 percent of control group II answered correctly compared to only 60 percent of the recipient group. No significant difference exists for the second question but still the sum of correct questions is significantly higher in control group II.

This would suggest that control group II has better knowledge from the outset. However, one has to point out that a problem of internal validity might contribute to this difference. The survey situation for the two groups was quite different putting the brochure recipients at a disadvantage. At the day of brochure distribution, the enumerators introduced themselves, and then, on the door step, confronted the recipients immediately with the test questions. Control group II, on the other hand, sat down with the enumerator and gave a full interview during which the three questions came up as part of a whole battery of questions. This gives rise to doubt whether the two results are comparable.

The mean results between the treatment group and the recipients who were not interviewed at follow-up show no significant differences. Here the interview situation was the same, so we can rule out that the group selected for the follow-up survey was better informed than those who received only the brochure.

Knowledge at Follow-up

Table 2.7 reports test results from the follow-up survey. It shows averages of correct test questions by treatment group in Columns 1-4 and p-values from t-tests of mean comparisons in Columns 5-8. We test for significant differences in means comparing brochure recipients to spill-over controls in Column 5. In Column 6 we compare recipients with controls from control villages and in Column 7 we report the p-value of a t-test between control I and control II respondents. In Column 8 we compare all campaign village respondents to those from control villages.

The item in the first row is a subjective and self-assessed measure that asked the respondents to rate how familiar they feel with issues of insurance. We observe a significant difference between brochure recipients and control group I. It is important to mention that these are often direct neighbors, yet 68 percent of the brochure recipients compared to only 57 percent of the non-recipients state to have an idea about how insurance works. The difference is statistically significantly at the 1 percent level. In contrast to this, there is no difference in this item between brochure recipients and controls from non-campaign villages. Two interpretations can be brought forward. Either, the randomization on the village level was unsuccessful, meaning the control II respondents had a higher subjective familiarity from the outset and the campaign had a positive impact which reduced the gap between brochure recipients and control II respondents to virtually zero. Or, the campaign had no effect at all or only a very small effect on the recipients' confidence and instead caused an unintended negative impact on the campaign village control group. In that case it was the fact of not receiving the informational material while some neighbors did, what significantly affected the subjective easiness towards insurance of control group I.

The items in Rows 2-13 are indicator variables for correct answers on the objective literacy test questions. Rows 14-17 show different indices constructed from these questions. *Index All* is the sum of correct answers from all knowledge questions. *Index Brochure* is the sum of correct answers to questions that had been explicitly explained in the brochure. *Index PhilHealth* sums up the correct answers on the questions concerning PhilHealth directly. The *Literacy index* is the sum of all but three questions that we found to be ambiguous[5].

The recipient group has on average 6.65 correct answers when all questions are considered and 5.13 when we exclude the ambiguous items. Both outcomes, the *Index All* and the *Literacy index*, are significantly higher for the brochure recipients (*assigned*) as compared to control group I. However, comparing brochure recipients and control group II does not reveal any difference in insurance knowledge. The impacts on subjective and objective knowledge could result from negative spill-over effects which undermines the hypothesis that social interaction is beneficial for the diffusion of insurance knowledge.

Test Scores by Gender

Table 2.8 displays the impact on insurance knowledge by gender of the respondent. For men and women, the self-assessed knowledge is lower in control group I than in the brochure group. The difference is larger among men with 14 percent

[5]Question 1: "No pay no coverage - If you have an insurance and you are late with your payments or do not pay, the insurance does not help you when you have a problem." This question is ambiguous since members of PhilHealth's Sponsored Program do not pay and are insured anyways. Question 2: "Inform insurer - If you have an insurance, you need to contact your insurer if something happens that is covered by the insurance." This question is ambiguous since claims under PhilHealth can be processed by the individual or by the hospital. This means that a PhilHealth member with valid membership information benefits automatically and it is the hospital who handles the claim with PhilHealth. Question 3: "PhilHealth availability - PhilHealth membership-cards are accepted in every hospital." This is an ambiguous question because even though PhilHealth membership is only valid in accredited hospitals almost all of the hospitals in the region are accredited.

compared to 10 percent among women. In both sub-samples the difference is highly statistically significant and indicates that non-recipients are less confident about insurance issues than brochure recipients. The objective test results are slightly higher for male respondents, which indicates a gender gap in knowledge. Further, for male respondents we cannot document the negative spill-over effect on knowledge.

Among female respondents the brochure recipients have significantly better results on the *Literacy index* and *Index All* than control group I. Again, comparing brochure recipients and controls from non-campaign villages shows no effect of the brochure. Apparently, female respondents drive the negative spill-over effects on insurance knowledge within campaign villages. We need to stress that the female sample is much larger than the male sample leading to more precise estimates.

The differences in the effects across gender raise the question why women who did not receive a brochure lose out by as much on the objective knowledge measures while we cannot observe a similar pattern for men. Male controls in campaign villages respond to not receiving a brochure with a significant decrease in the subjective measure, too, but not in the objective tests. In the case where information distortion explains the negative spill-over effect, one could argue that men, due to higher knowledge, are less susceptible to faulty information or that they are less exposed to it due to absence from the household during day time.

Test Scores by Insurance Status

The national social health insurer PhilHealth offers three different types of membership. Formally employed individuals are obliged by law to enroll under the Employed Sector Program (EP) and informal sector households can enroll voluntarily in the Individually Paying Program (IPP). Indigent households enroll in the Sponsored

Program (SP). While coverage and benefit structure are the same in all three programs the Sponsored Program is free of charge. Eligibility for this program depends on the household's income and living situation and is granted if the Department for Social Welfare and Development classifies the household in the lowest income quantile[6]. In the following we class membership into two types: *paying* members including formally employed and individually paying members and *non-paying* members including those with sponsored membership, i.e. either SP membership or IPP membership payed by someone else.

In Table 2.9 we observe that controls enrolled in PhilHealth, regardless whether paying or non-paying, have higher subjective familiarity with insurance and higher test scores than uninsured controls. We observe that regardless of insurance status the brochure recipients are more likely to state that they have an idea about insurance. For the objective test results, we also observe that control group I looses out as compared to the brochure group. Yet, the difference in the *Literacy Index* is only significant for the insured who are actually paying and not for the uninsured nor the insured-for-free sub-samples. It is important to mention here, that the precision of the mean estimates is much lower in all sub-samples.

When we look at brochure recipients and compare the level of self assessed familiarity across membership status, we see that among the uninsured around 57 percent state to have an idea about insurance compared to over 70 percent among the insured. Interestingly, a look at the objective knowledge tests reveals that the level of knowledge is quite similar for the uninsured who score on average 4.94 on the *Literacy index* and those who are insured for free, who score 4.98. This result is backed by anecdotal evidence from PhilHealth and barangay health center staff:

[6]Membership in the Sponsored Program is payed by PhilHealth and the local government or by philanthropic institutions or individuals.

Those insured for free are not familiar with the insurance product at their disposal and therefore do not use or, when they try to use it, make disappointing experiences as it does not cover the services they thought it would. Those who are insured and pay for it, on the other hand, score about 5.42 on average.

Estimation Strategy

We complement the means comparison by regression analysis taking into account individual characteristics of the respondents. Our estimations follow the model

$$Y_i = \alpha + \beta Brochure_i + \gamma Spillover_i + \theta X_i + u_i \tag{2.1}$$

where Y_i is the insurance literacy measure of interest, $Brochure_i$ is an indicator variable that captures whether the household has received a brochure and $Spillover_i$ takes value one for non-recipient households from campaign villages (control group I). In X_i we include the covariates age and gender of the household head, gender of the respondent and indicators for the household's income generating activities. All covariates included in the regressions where collected at the follow-up interview. We do not control for PhilHealth membership or type of PhilHealth membership since these were assessed post-treatment and might have been affected by the treatment.

Estimation Results

Table 2.10 presents OLS estimation results of equation 2.1. Receiving a brochure shows no significant effect on the *Literacy Index*, neither in the full sample (panel A) nor in the female sample (panel B). This gives evidence for the ineffectiveness of the treatment as to improving the target group's capacity to make informed decisions about issues of insurance. Alarming, however, are the coefficient estimates for the spill-over effect. Not receiving a brochure in a campaign village has a negative and

significant impact in most specifications. The size of this effect is small - one-third of a correct question in the female sample - its significance disappears after inclusion of additional covariates, i.e. an indicator whether recently a household member died or was confined to the hospital, ownership of a vehicle to capture wealth and the number of children. The results are very similar when we consider the impact on *Index All* in Table 2.11. As mentioned earlier, *shock history* might potentially be endogenous as brochure distribution might have affected the recall behavior.

Table 2.12 displays coefficient estimates from linear probability models where the specific knowledge items are plugged in as dependent variables. Only few coefficients are significantly different from zero, but we observe that eight out of twelve estimations result in negative estimates of the spill-over effect. If we estimate these coefficients on basis of the female sample we obtain negative estimates for ten of the twelve items (Table 2.13). In the regressions with "Know 4 - Inform Insurer" and "Know 5 - Money Back" as respective dependent variable, the brochures' impact and the spill-over coefficient are negative and significantly different from zero.

Heterogeneous Effects

Simple analysis of mean results potentially hides changes that occur in the distribution of the test results. Figure 2.1 presents kernel density estimate of the distribution. It shows the number of correct knowledge test questions on the x-axes and the share of respondents who scored that many correct questions on the y-axes. The most frequent number of correct questions was five for the brochure recipients and control group I. For control group II the mode is four but the share of respondents scoring five correct questions is only marginally smaller. When it comes to scores above six the brochure recipients have strictly higher fractions than both control

groups. The general picture indicates a shift of the recipient distribution towards higher test scores. We do not report here probit regressions that we ran to test whether an impact occurred on the probability to score above six correct knowledge questions. These regressions resulted in insignificant effects and, thus, did not confirm the hypothesis that brochure recipients scored better on the higher end of the test score distribution, as the right shift in the distribution would suggest.

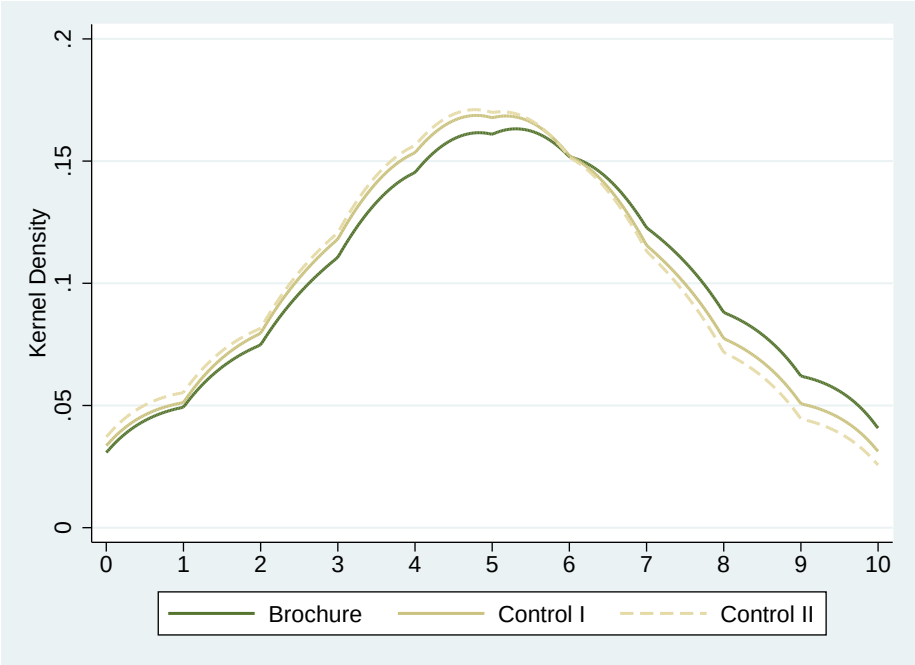

Figure 2.1: Distribution of Literacy index in different treatment groups

Discussion of Spill-over Effect on Knowledge

The negative externalities that the campaign potentially created for non-participants cannot be ignored. We offer two explanations for the negative spill-over effect: lack of confidence and social interaction. The first explanation is based on the respondent's subjective assessment of knowledge. Non-recipients significantly assess their knowledge to be lower compared to brochure recipients. They show a lack of confidence that results from the fact that they had not been selected to receive the brochure.

This lack of self-esteem might influence their capacity to respond correctly to the test questions. The literature on *stereotype threat* documents a similar phenomenon. Hoff and Pandey (2004) find that affiliation to a group that is stigmatized to be less knowledgeable reduces scores in objective tests. The mechanism they identified might be similar to what we observe in our study. Public knowledge of affiliation to the stigmatized group affects performance in a cognitive task.

Our interview protocol gave the instruction to start interviews of control group I respondents always with the question whether the household had received a brochure the week before. In this way affiliation to a group became *public*. Other than in the *stereotype threat* literature we do not control for the setting in which the interview was conducted. We are not aware if the interviewer was alone with the respondent or in company of family, friends and neighbors. One might suspect heterogeneity according to the setting. However, in this regard no structural differences should exist by treatment status.

The second explanation focuses on social interaction. When recipients and non-recipients discuss the content of the brochure, misconception might arise due to the treatment group's incapacity to explain issues of insurance correctly. Especially, brochure recipients unfamiliar with the issue might give incorrect summaries of what is presented in the brochure. As a consequence, the mere forwarding of the brochure's content causes the observed distortion that puts non-recipients at a disadvantage. Again, we cannot test this hypothesis, since we have only incomplete information on whether control group I obtained information on the brochure.

2.5.2 The Impact on Attitude

In the following we analyze the impact on attitude towards insurance. We assess attitude using the concept of semantic differential. The respondents rated 19 pairs of bipolar adjectives on a scale ranging from -2 to 2[7]. The adjectives referred either to "Insurance in general" or to "Buying health insurance". The mean comparisons are presented in Table 2.16. For reasons of space the table lists only one adjective of the bipolar pair, i.e. the one that was coded with value 2, while the opposing adjective, coded with -2, is omitted here. Items one to twelve refer to "Insurance in general" and items 13 to 19 refer to "Buying Health Insurance"[8].

Attitude at Day of Campaign

Table 2.14 presents average results for attitude on the day of the campaign. Column 5 gives the average score for all households that received a brochure, Column 6 gives the average for recipient households that were also interviewed at follow-up and that constitute now our treatment group. Column 7 lists the results for the group of recipients that was not selected for follow-up and, for comparison, we include in Column 8 the average of control group II, that was not exposed to spill-over effects. It shows that the three items asked at distribution are well balanced across the different groups. All groups rate insurance in general as something rather positive, rather simple and valuable. Control group II has the same perception at the full survey.

[7]E.g. positive (2) - rather positive (1) - neither positive nor negative (0) - rather negative (-1) - negative (-2).

[8]The exact survey questions were "How do you feel towards insurance in general?" and "How do you feel towards buying health insurance?" See the original questions in section 2.9

Empirical Analysis

The large number of attitude variables makes it difficult to interpret the impact of the campaign in a sensible manner. To overcome this problem, we apply a factor analysis (FA) that determines underlying, latent variables, called factors[9]. This method attempts to explain the covariation that we observe in our attitude variables, then groups variables with similar variation into sets according to a latent factor, and thereby reduces the dimensionality of the data and renders interpretation easier. For person i we have,

$$y_{ij} = \lambda_{j1} f_{i1} + \lambda_{j2} f_{i2} + ... + \lambda_{jk} f_{ik} + e_{ij} \tag{2.2}$$

where y_{ij} is the observed information that respondent i gave on the $j = 1, ..., p$ questions, f_{ik} are the $k = 1, ..., m$ latent, unobserved factors, and λ_{jk} are the respective loadings of factor f_{ik} for variable y_{ij}. The e_{ij} are residuals that have zero mean and are not correlated to the factors. In this fashion, we run two separate factor analyses, one for the first twelve items capturing the attitude towards "Insurance in general" and the other one for the remaining seven items that assess the attitude towards "Buying health insurance".

We identify three factors that account for most of the variance in the items on "Insurance in general"[10]. Table 2.17 shows the loading of each variable on the three different factors. A higher absolute loading indicates greater importance of the variable for the factor and a positive sign indicates a positive relation between the factor and the variable. As above, the listed adjective takes on value 2 and the omitted

[9]See Kim and Mueller (1978) for a detailed description of factor analysis.

[10]As suggested by the literature on factor analysis we select only those factors with Eigenvalue greater than one.

opposite takes on value -2. For factor 1 the most relevant pairs of adjectives are "unfair - fair", "unreliable - reliable", "unnecessary - necessary", "risky - safe" and "wasteful - beneficial". We interpret this factor as capturing issues of *security*. Factor 2 is mostly defined by "positive - negative", "poor - rich", "worthless - valuable", "complicated - simple" and "powerless - powerful". The interpretation of this factor is not straightforward as these variables have no "common denominator", which leads us to label the factor as *diffuse*. Factor 3 is most straightforward to interpret as the variables with highest loadings, "unaffordable - affordable" and "expensive - cheap", obviously capture financial issues. We modify the factor loadings by setting the loadings to zero for all variables that have negligible loading for the specific factor. This reduces variation after having determined the underlying factors. This is an arbitrary decision but given that we do so for all three groups it does not affect internal validity of our experiment.

After identification of the latent factors, we turn to the estimation of the campaign's impact on attitude. We estimate Equation 2.1 separately for each modified factor as dependent variable. Table 2.19 shows that we obtain highly significant estimates for the brochure's effect and for the spill-over effect. For all factors, the two effects point in the same direction leading us to conclude that the campaigning has affected the attitude within campaign villages similarly, regardless of whether a household received a brochure or not.

The estimates of both, the brochure's direct impact and the spill-over effect on Factor 1, are positive and significant. This indicates that the campaign increased the general perception of insurance being something beneficial, security enhancing. It is important to mention that the spill-over effect is larger in magnitude than the direct effect on most factors. One could interpret this as the control I respondents com-

pensation for their lower subjective knowledge. Factor 3 for "Insurance in general" has highest loading for "affordable" and "cheap" and is affected negatively by the intervention. This means, as a result of the treatment, campaign village respondents are less inclined towards these adjectives when rating the financial side of insurance. The negative estimates suggest an increased reluctance to underrate the financial commitment of insuring oneself. In other words, one could conclude that brochure distribution has raised the recipients' cautiousness as to the financial commitment an insurance contract brings about.

The FA on the semantic differential items for "Buying health insurance" results in two factors (Table 2.18). For items "good", "wise" and "useful" factor 1 has the highest loadings. For variables "easy" and "close" it is factor 2. The first factor captures the worthiness of buying health insurance, the second captures issues of accessibility. Regression analyses of these factors on brochure assignment, the spillover and other covariates are presented in Table 2.21.

For "Buying Health Insurance" the worthiness factor was affected significantly positive. For factor 2, on the other hand, brochure and spillover effect have negative coefficients. This suggests that the feeling of how accessible health insurance is, was affected negatively. Putting this together one could conclude that the campaign created an increased perception of how useful health insurance is, but also raised awareness about the lack of accessibility to insurance.

In Table 2.21 we look at the effect on the specific attitude items. The accessibility factor has highest loading for the pair of adjectives "difficult - easy". The impact on this item is negative but insignificant for both, the direct and the spill-over effect. For the second item of this factor, "far-close", we estimate negative impacts as well, now with a significant impact on brochure recipients only.

2.5.3 The Impact on Insurance Take-up

We assess the effect of the campaign on actual behavior by looking at take-up of two PhilHealth products designed for the low-income sector: The Individually Paying Program (IPP) and the Sponsored Program (SP). We use PhilHealth's administrative records on new registrations for these two programs. The data covers the period of six months succeeding the campaign. The data is not restricted to our experimental villages, it also contains villages where we did not distribute the brochure or conduct interviews. In order to avoid confusion we call the control II villages henceforth "interview villages" and the additional set of villages "pure control villages".

Including this group of control villages has two advantages: When estimating the impact on take-up on the barangay level we can improve the power of our tests by increasing the number of controls above the number of villages comprised by our set of "interview" villages (Duflo et al., 2008). Second, even though we did not distribute brochures in the control II villages it is mere interviewing by itself that might affect subsequent behavior which could bias our estimates (Zwane et al., 2011). At least on the barangay level we can avoid this source of bias by comparing campaign and unaffected control villages.

Inclusion of the control villages also has a drawback. As pointed out in Section 2.3 the experimental villages, i.e. the campaign and interview villages, where selected for their accessibility. For the pure control villages data availability does not allow us to identify how remote those villages are. Consequently, it can not be ruled out that the experimental sites and the pure control villages are inherently different in this indicator. We are only able to exclude villages that are of different population size than our initial sample.

In the following, we first estimate the campaign's effect on the village level using administrative data exclusively. Then we estimate the impact on the individual level where we match the recipient, control I and control II respondents to the administrative data.

Take-up on Barangay Level

PhilHealth registered 353 new members in the campaign villages, 280 new members in the interview villages and 755 new members in the 197 pure control villages (Table 2.22). Around 41 percent of the new registrations in the campaign villages are new SP members. In the interview villages this share is slightly higher at 48.5 percent and in the pure control villages it is at only 8 percent.

Table 2.23 shows averages by type of village. We observe on average ten new members in the experimental villages, i.e. the campaign and interview villages, and only 4.3 new members in the pure control villages. Given that the set of control villages might potentially be very different from the experimental sites we need to stress that the significant difference likely suffers from bias. In the experimental villages we see that 6 of the ten new enrollees are in the IPP and 4 in the SP.

Using data on barangay population size we calculate take-up rates, i.e. the share of new enrollees in the barangay population[11]. The take-up rates are below 1 percent for all products. For IPP the take- up rate in campaign and inerview villages are significantly different from the pure control villages. The SP take-up in campaign villages is significantly different as compared to the pure control villages. Despite this, the magnitude of the coefficient and of the difference in means rules out economic significance. Though not displayed in the table, any t-test comparing take-up between campaign and interview villages results in insignificant differences.

[11]Source: http://www.nscb.gov.ph

Take-up on Individual Level

We use the individual-level data from our follow-up survey and individual records of new PhilHealth members to assess the impact on the individual level. A main obstacle is the matching of our survey data with the administrative records. We apply the `soundex`-algorithm implemented in STATA to identify the names of brochure recipients and control respondents from our survey in the administrative data.

Out of our 711 respondents not enrolled with PhilHealth at the time of the follow-up we are able to match 2 percent as newly registered members in the six months following the campaign. Table 2.24 presents the average take-up rates by treatment status. Among brochure recipients and control I respondents alike the take-up rate of 1 percent is significantly lower than it is in the control II sample where it is at 4 percent. Again, SP take-up is more important than IPP take-up. This result suggests that "interviewing" has an even higher impact than brochure distribution. However, the regression analyses show that this effect is not robust as significance vanishes once we control for individual characteristics. We estimate different specifications. Tables 2.25-2.27 present linear probability Models (LPM) estimates and logistic regression results with total take-up, IPP and SP take-up as dependent variable. We run the regression on a sample consisting only of respondents from campaign villages (TT=1 w/o T=0) and on a sample consisting only of brochure recipients and control II respondents from interview villages (T=1 w/o TT=0). Neither LPM nor logistic specifications produce significant coefficients. Further, we estimate random-effects panel models (LPM and logistic) in Tables 2.28-2.30 that fail equally to provide any evidence for an impact on take-up of the two products.

2.6 Conclusion

This evaluation exercise analyzes whether insurance knowledge distribution in the general public can affect knowledge of, attitude towards and take-up of insurance. Our randomized experiment cannot provide evidence for a positive effect on knowledge in the short term on treated households. In addition to this, a second downside to the campaign are negative spill-over effects on the subjective and objective easiness with insurance of untreated households in campaign villages. While treated households do not fare better in the test results than an unaffected control population, the campaign village controls end up having lower self-rated comprehension of issues of insurance and lower test results.

We propose two explanations for the emergence of the unintended negative effects on test scores. First, anticipation of control households to fare worse in the tests self fulfills. And second, after the campaign, information diffuses in the village and incorrect statements spread which affect untreated households. While our paper fails to provide detailed insight into the process causing the negative spill-over, it calls for caution on unintended spillover effects caused by social interaction. From a methodological point of view, our results show that such effects, if not explicitly investigated, can influence experimental results and lead to wrong conclusions.

From an ethical point of view, our study shows that there is an important need to assess the effects that interventions can have on untreated subjects. A phenomenon similar to our unintended negative spill-over on knowledge is documented by Kremer and Miguel (2007). They report that more direct social ties to people that used deworming drugs reduced attitude among people who were not treated initially. We conclude that the effects created by social interaction can lie beyond the studies'

realm of observation. Obviously, when treated and untreated individuals interact any impact assessment needs to incorporate efforts to capture unintended spill-over effects rigorously.

The positive impact on attitude that we observe equally calls for caution. The inefficiency of financial education interventions in developing countries coupled with their positive effect on perception might cause uninformed decision making. A priori, this is not an undesirable result. A policymaker keen on improving take-up of microinsurance might want to rely on insurance education to reap the advantages of improved perception despite the lack of improvement in testable short-term knowledge. If however customer protection is weak and if the target population lacks access to institutions where complaints can be settled uninformed take-up might be disadvantageous.

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Appendix

Table 2.2: Summary statistics by group

	Campaign villages					
	All (T=1) (1)	Brochure HH (TT=1) (2)	Control I (TT=0) (3)	Control II (T=0) (4)	p-value of t-test (2)-(3)	(2)-(4)
Age head	41.50	41.55	41.37	41.34	0.69	0.67
(se)	0.20	0.24	0.39	0.44		
Female head	0.11	0.11	0.12	0.07	0.53	0.03
(se)	0.01	0.01	0.01	0.01		
Schooling head	8.17	8.20	8.12	8.55	0.61	0.04
(se)	0.07	0.08	0.13	0.15		
Schooling spouse	8.64	8.69	8.52	8.70	0.23	0.94
(se)	0.06	0.07	0.12	0.15		
No of children	2.84	2.84	2.82	3.03	0.83	0.05
(se)	0.04	0.05	0.08	0.09		
PhilHealth member	0.73	0.73	0.72	0.74	0.58	0.50
(se)	0.01	0.01	0.02	0.02		
Paying for Philhealth	0.37	0.37	0.36	0.35	0.67	0.36
(se)	0.01	0.01	0.02	0.02		
Bank account	0.10	0.10	0.08	0.10	0.13	0.76
(se)	0.01	0.01	0.01	0.01		
Landowner	0.30	0.29	0.31	0.31	0.34	0.43
(se)	0.01	0.01	0.02	0.02		
Mobile phone	0.62	0.63	0.58	0.62	0.02	0.63
(se)	0.01	0.01	0.02	0.02		
Motor cycle/car	0.19	0.19	0.18	0.21	0.40	0.38
(se)	0.01	0.01	0.02	0.02		
Selfemployed	0.48	0.48	0.46	0.49	0.41	0.87
(se)	0.01	0.01	0.02	0.02		
Permanent	0.27	0.26	0.29	0.29	0.21	0.27
(se)	0.01	0.01	0.02	0.02		
Remittances abroad	0.04	0.04	0.04	0.05	0.94	0.35
(se)	0.00	0.01	0.01	0.01		
Contact w/ officials	0.88	0.89	0.86	0.82	0.09	0.00
(se)	0.01	0.01	0.01	0.02		
Shock history	0.32	0.34	0.26	0.31	0.00	0.17
(se)	0.01	0.01	0.02	0.02		
Observations	2095	1505	590	447		

Note: Column (1) includes all respondents from campaign villages. Column (2) includes only brochure recipients. Column (3) includes controls from campaign villages. Column (4) includes households from control villages. Columns (5) and (6) present p-values of t-test comparing brochure recipients with the two control groups, respectively.

Table 2.3: Summary statistics by group (Non-PhilHealth households)

	Campaign villages					
	All (T=1) (1)	Brochure HH (TT=1) (2)	Control I (TT=0) (3)	Control II (T=0) (4)	p-value of t-test (2)-(3)	(2)-(4)
Age head	40.63	40.54	40.86	41.11	0.72	0.58
(se)	0.41	0.47	0.78	0.97		
Female head	0.17	0.16	0.19	0.04	0.30	0.00
(se)	0.02	0.02	0.03	0.02		
Schooling head	8.02	7.97	8.14	7.98	0.55	0.97
(se)	0.13	0.15	0.24	0.27		
Schooling spouse	8.25	8.23	8.30	8.60	0.81	0.21
(se)	0.12	0.14	0.24	0.28		
No of children	2.55	2.51	2.67	2.68	0.35	0.39
(se)	0.08	0.09	0.15	0.17		
Bank account	0.07	0.07	0.07	0.06	0.93	0.80
(se)	0.01	0.01	0.02	0.02		
Landowner	0.28	0.27	0.32	0.27	0.23	0.92
(se)	0.02	0.02	0.04	0.04		
Mobile phone	0.55	0.59	0.45	0.62	0.00	0.58
(se)	0.02	0.02	0.04	0.05		
Motor cycle/car	0.16	0.16	0.14	0.20	0.46	0.33
(se)	0.02	0.02	0.03	0.04		
Selfemployed	0.45	0.44	0.47	0.52	0.53	0.13
(se)	0.02	0.02	0.04	0.05		
Permanent	0.23	0.23	0.22	0.16	0.91	0.11
(se)	0.02	0.02	0.03	0.03		
Remittances abroad	0.05	0.05	0.06	0.04	0.76	0.51
(se)	0.01	0.01	0.02	0.02		
Contact w/ officials	0.85	0.86	0.82	0.82	0.17	0.27
(se)	0.02	0.02	0.03	0.04		
Shock history	0.26	0.27	0.24	0.24	0.53	0.52
(se)	0.02	0.02	0.03	0.04		
Observations	575	409	166	114		

Note: The table includes only households not enrolled with PhilHealth at follow-up interview. The first column (T=1) includes all respondents from campaign villages. The second column (TT=1) includes only brochure recipients. The third column (TT=0) includes controls from campaign villages. The fourth column (T=0) includes households from control villages. Columns (5) and (6) present p-values of t-tests comparing brochure recipients with controls either from campaign villages or from control villages, respectively.

Table 2.4: Summary statistics by group (Female sample)

	Campaign villages					
	All (T=1) (1)	Brochure HH (TT=1) (2)	Control I (TT=0) (3)	Control II (T=0) (4)	p-value of t-test (2)-(3)	(2)-(4)
Age head	41.82	41.78	41.92	42.01	0.79	0.69
(se)	0.24	0.28	0.43	0.51		
Female head	0.15	0.15	0.14	0.10	0.66	0.01
(se)	0.01	0.01	0.02	0.02		
Schooling head	8.12	8.12	8.12	8.55	0.98	0.02
(se)	0.08	0.09	0.14	0.17		
Schooling spouse	8.59	8.66	8.46	8.86	0.20	0.24
(se)	0.07	0.09	0.13	0.16		
No of children	2.95	2.96	2.94	3.06	0.84	0.39
(se)	0.05	0.06	0.08	0.10		
PhilHealth member	0.72	0.72	0.71	0.74	0.15	0.83
(se)	0.01	0.01	0.02	0.02		
Paying for PhilHealth	0.37	0.37	0.36	0.36	0.31	0.31
(se)	0.01	0.01	0.02	0.03		
Bank account	0.10	0.11	0.08	0.10	0.15	0.83
(se)	0.01	0.01	0.01	0.02		
Landowner	0.29	0.28	0.31	0.31	0.31	0.31
(se)	0.01	0.01	0.02	0.03		
Mobile phone	0.63	0.65	0.59	0.63	0.01	0.48
(se)	0.01	0.01	0.02	0.03		
Motor cycle/car	0.18	0.19	0.17	0.22	0.49	0.17
(se)	0.01	0.01	0.02	0.02		
Selfemployed	0.47	0.48	0.45	0.47	0.38	0.68
(se)	0.01	0.02	0.02	0.03		
Trade	0.24	0.25	0.21	0.18	0.08	0.00
(se)	0.01	0.01	0.02	0.02		
Permanent	0.29	0.28	0.31	0.32	0.20	0.09
(se)	0.01	0.01	0.02	0.03		
Remittances abroad	0.05	0.05	0.04	0.05	0.80	0.61
(se)	0.01	0.01	0.01	0.01		
Contact w/ officials	0.88	0.88	0.86	0.83	0.22	0.02
(se)	0.01	0.01	0.02	0.02		
Shock history	0.31	0.35	0.25	0.29	0.00	0.05
(se)	0.01	0.01	0.02	0.02		
Observations	1557	1068	489	337		

Note: Column (1) includes all respondents from campaign villages. Column (2) includes only brochure recipients. Column (3) includes controls from campaign villages. Column (4) includes households from control villages. Columns (5) and (6) present p-values of t-test comparing brochure recipients with the two control groups, respectively.

Table 2.5: Knowledge at Delivery

	Number of Observations				Mean Score						
	All recipients	Follow-up recipients	Lost recipients	Control II	All recipients (1)	Follow-up recipients (2)	Lost recipients (3)	Control II (4)	p-value of t-test (1)-(4)	(2)-(4)	(3)-(4)
Freq. payment	1784	1276	508	459	.81	.81	.82	.85	.07	.06	.23
Claim	1788	1282	506	461	.64	.64	.65	.62	.44	.49	.45
Premium	1794	1286	508	460	.6	.6	.59	.75	.00	.00	.00
Before index	1772	1268	504	458	2.06	2.06	2.07	2.22	.00	.00	.01

Note: This table compares knowledge at the time of brochure distribution for all brochure recipients, recipients that were followed up, and recipients that were not followed up. It also includes the results of control group II for the respective knowledge items.
Before index is the sum of scores on the items positive-valuable-simple

Table 2.6: Knowledge at Delivery (Female sample)

	Number of Observations		Mean Score		
	Follow-up recipients	Control II	Follow-up recipients (1)	Control II (2)	p-value of t-test (1)-(2)
Freq. payment	857	347	.78	.83	.03
Claim	864	349	.62	.6	.72
Premium	866	348	.55	.72	.00
Before index	851	346	1.95	2.16	.00

Note: This table compares attitude at the time of the delivery for female brochure recipients with female Control II respondents. The attrition sample is omitted since gender is only available in the .
Before index is the sum of scores on the items Frequency of payment, Claim, Premium

Table 2.7: Knowledge - by Treatment Group

	Campaign villages							
	All (T=1) (1)	Brochure HH (TT=1) (2)	Control I (TT=0) (3)	Control II (T=0) (4)	p-value of t-test (2)-(3)	(2)-(4)	(3)-(4)	(1)-(4)
1 Idea about insurance (0/1)	0.65	0.68	0.57	0.67	0.00	0.81	0.00	0.34
Std error	0.01	0.01	0.02	0.02				
2 Insurance is free (0/1)	0.34	0.34	0.34	0.34	0.95	0.83	0.89	0.83
Std error	0.01	0.01	0.02	0.02				
3 No regular fee (0/1)	0.52	0.52	0.50	0.52	0.50	0.94	0.65	0.92
Std error	0.01	0.01	0.02	0.02				
4 No pay no coverage (0/1)	0.56	0.54	0.60	0.55	0.02	0.92	0.09	0.61
Std error	0.01	0.01	0.02	0.02				
5 Inform insurer (0/1)	0.87	0.87	0.87	0.91	0.92	0.01	0.03	0.01
Std error	0.01	0.01	0.01	0.01				
6 Money back (0/1)	0.37	0.39	0.33	0.38	0.01	0.86	0.07	0.64
Std error	0.01	0.01	0.02	0.02				
7 Life insurance (0/1)	0.69	0.69	0.67	0.68	0.24	0.57	0.67	0.79
Std error	0.01	0.01	0.02	0.02				
8 PhilHealth availability (0/1)	0.12	0.11	0.13	0.14	0.24	0.05	0.48	0.09
Std error	0.01	0.01	0.01	0.02				
9 PhilHealth coverage (0/1)	0.35	0.36	0.33	0.32	0.25	0.11	0.62	0.17
Std error	0.01	0.01	0.02	0.02				
10 Frequency payment (0/1)	0.84	0.86	0.81	0.85	0.01	0.53	0.13	0.94
Std error	0.01	0.01	0.02	0.02				
11 Claim (0/1)	0.60	0.60	0.57	0.63	0.19	0.44	0.10	0.26
Std error	0.01	0.01	0.02	0.02				
12 Premium (0/1)	0.73	0.74	0.70	0.75	0.04	0.58	0.04	0.27
Std error	0.01	0.01	0.02	0.02				
13 Typhoon coping (0/1)	0.63	0.64	0.61	0.64	0.18	0.83	0.23	0.57
Std error	0.01	0.01	0.02	0.02				
Index all (0-12)	6.60	6.65	6.44	6.74	0.04	0.45	0.03	0.18
Std error	0.05	0.05	0.09	0.10				

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... table 2.7 continued

	Campaign villages							
	All (T=1) (1)	Brochure HH (TT=1) (2)	Control I (TT=0) (3)	Control II (T=0) (4)	p-value of t-test (2)-(3)	(2)-(4)	(3)-(4)	(1)-(4)
Index brochure (0-5)	1.78	1.78	1.77	1.80	0.71	0.83	0.65	0.76
Std error	0.02	0.03	0.05	0.05				
Index PhilHealth (0-2)	0.47	0.47	0.46	0.46	0.76	0.86	0.93	0.92
Std error	0.01	0.02	0.03	0.03				
Literacy index (0-9)	5.06	5.13	4.85	5.10	0.00	0.77	0.03	0.63
Std error	0.04	0.05	0.08	0.09				
Observations	2090	1506	584	448				

Note: This table provides average scores on insurance knowledge test questions. See Figure 2.8 for the original questions.

Table 2.8: Knowledge - by Gender

	Male sample					Female sample				
	Brochure HH (TT=1)	Control I (TT=0)	Control II (T=0)	p-value of t-test		Brochure HH (TT=1)	Control I (TT=0)	Control II (T=0)	p-value of t-test	
	(1)	(2)	(3)	(1)-(2)	(1)-(3)	(6)	(7)	(8)	(6)-(7)	(6)-(8)
1 Idea about insurance (0/1)	0.68	0.54	0.72	0.01	0.41	0.68	0.58	0.66	0.00	0.51
Std error	0.02	0.05	0.04			0.01	0.02	0.03		
2 Insurance is free (0/1)	0.34	0.40	0.31	0.27	0.50	0.33	0.33	0.35	0.76	0.50
Std error	0.02	0.05	0.04			0.01	0.02	0.03		
3 No regular fee (0/1)	0.50	0.49	0.44	0.74	0.22	0.53	0.51	0.54	0.60	0.61
Std error	0.02	0.05	0.05			0.02	0.02	0.03		
4 No pay no coverage (0/1)	0.57	0.58	0.57	0.81	0.99	0.53	0.61	0.54	0.01	0.84
Std error	0.02	0.05	0.05			0.02	0.02	0.03		
5 Inform insurer (0/1)	0.87	0.83	0.89	0.26	0.61	0.87	0.88	0.92	0.54	0.01
Std error	0.02	0.04	0.03			0.01	0.01	0.01		
6 Money back (0/1)	0.35	0.26	0.27	0.08	0.10	0.40	0.34	0.42	0.02	0.53
Std error	0.02	0.04	0.04			0.02	0.02	0.03		
7 Life insurance (0/1)	0.69	0.76	0.74	0.19	0.35	0.69	0.65	0.66	0.07	0.22
Std error	0.02	0.04	0.04			0.01	0.02	0.03		
8 PhilHealth availability (0/1)	0.11	0.15	0.17	0.29	0.08	0.11	0.12	0.13	0.49	0.29
Std error	0.01	0.03	0.04			0.01	0.01	0.02		
9 PhilHealth coverage (0/1)	0.38	0.36	0.31	0.66	0.14	0.35	0.32	0.32	0.23	0.31
Std error	0.02	0.05	0.04			0.01	0.02	0.03		
10 Frequency payment (0/1)	0.89	0.86	0.90	0.41	0.73	0.84	0.80	0.83	0.05	0.46
Std error	0.01	0.03	0.03			0.01	0.02	0.02		
11 Claim (0/1)	0.59	0.68	0.68	0.10	0.07	0.61	0.55	0.61	0.03	0.88
Std error	0.02	0.05	0.04			0.02	0.02	0.03		
12 Premium (0/1)	0.78	0.76	0.82	0.61	0.41	0.72	0.68	0.73	0.08	0.86
Std error	0.02	0.04	0.04			0.01	0.02	0.02		
13 Typhoon coping (0/1)	0.66	0.67	0.64	0.84	0.70	0.63	0.59	0.64	0.16	0.60
Std error	0.02	0.05	0.05			0.01	0.02	0.03		
Index all (0-12)	6.74	6.76	6.77	0.90	0.85	6.62	6.37	6.72	0.03	0.44
Std error	0.09	0.21	0.19			0.06	0.10	0.11		
Index brochure (0-5)	1.76	1.72	1.59	0.70	0.13	1.80	1.78	1.87	0.85	0.29

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... table 2.8 continued

	Male sample					Female sample				
	Brochure HH (TT=1)	Control I (TT=0)	Control II (T=0)	p-value of t-test		Brochure HH (TT=1)	Control I (TT=0)	Control II (T=0)	p-value of t-test	
	(1)	(2)	(3)	(1)-(2)	(1)-(3)	(6)	(7)	(8)	(6)-(7)	(6)-(8)
Std error	0.05	0.11	0.09			0.03	0.05	0.06		
Index PhilHealth (0-2)	0.49	0.50	0.47	0.82	0.82	0.46	0.44	0.46	0.55	0.88
Std error	0.03	0.07	0.07			0.02	0.03	0.04		
Literacy index (0-9)	5.20	5.23	5.11	0.85	0.65	5.11	4.77	5.10	0.00	0.93
Std error	0.09	0.18	0.18			0.06	0.09	0.10		
Observations	455	103	114			1050	487	333		

Note: This table provides average scores on insurance knowledge test questions. See Figure 2.8 in the Appendis for the original questions.

Table 2.9: Knowledge - by PhilHealth Status

	Uninsured sample					Insured for free					Insured paying				
				P-value of					P-value of					P-value of	
	(TT=1)	(TT=0)	(T=0)	(1)-(2)	(1)-(3)	(TT=1)	(TT=0)	(T=0)	(4)-(5)	(4)-(6)	(TT=1)	(TT=0)	(T=0)	(7)-(8)	(7)-(9)
	(1)	(2)	(3)			(4)	(5)	(6)			(7)	(8)	(9)		
Idea ins.	0.57	0.43	0.59	0.00	0.80	0.71	0.63	0.73	0.04	0.47	0.73	0.63	0.66	0.01	0.12
Std error	0.02	0.04	0.05			0.02	0.03	0.03			0.02	0.03	0.04		
Index all	6.46	6.15	6.51	0.10	0.82	6.55	6.44	6.74	0.51	0.27	6.89	6.68	6.90	0.21	0.99
Std error	0.10	0.16	0.19			0.08	0.15	0.16			0.09	0.14	0.16		
Index B.	1.77	1.66	1.71	0.30	0.62	1.71	1.70	1.78	0.96	0.48	1.87	1.91	1.88	0.68	0.89
Std error	0.05	0.09	0.10			0.05	0.08	0.09			0.05	0.08	0.09		
Index P.	0.42	0.45	0.39	0.59	0.61	0.47	0.50	0.49	0.46	0.66	0.51	0.42	0.49	0.10	0.73
Std error	0.03	0.05	0.06			0.03	0.05	0.05			0.03	0.04	0.05		
Literacy	4.94	4.67	4.96	0.12	0.94	4.98	4.79	5.02	0.21	0.81	5.42	5.06	5.31	0.02	0.51
Std error	0.10	0.14	0.18			0.08	0.13	0.14			0.08	0.13	0.15		
N	409	166	114			537	209	177			560	209	157		

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Table 2.10: Linear Regression - Brochure Impact and Spill-over Effect on Knowledge

	(1)	(2)	(3)	(4)	(5)	(6)
	PANEL A: Full Sample					
Brochure received	0.03	0.04	0.04	0.03	0.04	0.04
	(0.10)	(0.10)	(0.10)	(0.12)	(0.12)	(0.10)
Spill-over	-0.26**	-0.22*	-0.16	-0.26*	-0.22	-0.16
	(0.12)	(0.12)	(0.12)	(0.14)	(0.14)	(0.12)
Observations	2571	2550	2532	2571	2550	2532
	PANEL B: Female Sample					
Brochure received	-0.00	0.04	0.05	-0.00	0.04	0.05
	(0.12)	(0.12)	(0.12)	(0.15)	(0.16)	(0.12)
Spill-over	-0.34**	-0.29**	-0.19	-0.34*	-0.29*	-0.19
	(0.13)	(0.13)	(0.14)	(0.17)	(0.17)	(0.14)
Observations	1894	1884	1867	1894	1884	1867
Control variables	no	yes	yes	no	yes	yes
Additional control variables	no	no	yes	no	no	yes
Robust standard errors	yes	yes	yes	no	no	no
Std. errors clustered at the barangay level	no	no	no	yes	yes	yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

a. Dependent variable is *Literacy Index*, i.e. the sum of correct insurance literacy questions at follow-up (excluding ambiguous questions).

b. Regressions with control variables control for gender of respondent, age and schooling of head, number of children, type of income source.

c. Additional control variables are shock history, ownership of motorcycle/car and population level of barangay.

Table 2.11: Linear Regression - Brochure Impact and Spill-over Effect on Knowledge (including ambiguous questions)

	(1)	(2)	(3)	(4)	(5)	(6)
	PANEL A: Full Sample					
Brochure received	-0.08	-0.08	-0.07	-0.08	-0.08	-0.07
	(0.11)	(0.11)	(0.11)	(0.11)	(0.12)	(0.10)
Spill-over	-0.29**	-0.26*	-0.19	-0.29**	-0.26*	-0.19
	(0.13)	(0.13)	(0.13)	(0.14)	(0.14)	(0.13)
Observations	2547	2526	2508	2547	2526	2508
	PANEL B: Female Sample					
Brochure	-0.11	-0.08	-0.05	-0.11	-0.08	-0.05
	(0.13)	(0.13)	(0.13)	(0.16)	(0.15)	(0.13)
Spill-over	-0.35**	-0.31**	-0.20	-0.35**	-0.31*	-0.20
	(0.15)	(0.15)	(0.15)	(0.18)	(0.17)	(0.16)
Observations	1873	1863	1846	1873	1863	1846
Control variables	no	yes	yes	no	yes	yes
Additional control variables	no	no	yes	no	no	yes
Robust standard errors	yes	yes	yes	no	no	no
Std. errors clustered at the barangay level	no	no	no	yes	yes	yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

a. Dependent variable is *Literacy Index*, i.e. the sum of correct insurance literacy questions at follow-up (excluding ambiguous questions).

b. Regressions with control variables control for gender of respondent, age and schooling of head, number of children, type of income source.

c. Additional control variables are shock history, ownership of motorcycle/car and population level of barangay.

Table 2.12: Linear Regression - Impact of Brochure and Spill-over on Knowledge-Items

	Know 1	Know 2	Know 3*	Know 4*	Know 5	Know 6	Know 7*	Know 8	Know 9	Know 10	Know 11	Know 12
Brochure	-0.00	0.00	-0.00	-0.05***	0.01	0.01	-0.03	0.04	0.01	-0.02	-0.01	-0.01
	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)
Spill-over	0.00	-0.01	0.06**	-0.05**	-0.05	-0.01	-0.01	0.01	-0.02	-0.05	-0.04	-0.04
	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)
Observations	2550	2550	2536	2547	2550	2550	2540	2550	2550	2550	2550	2550

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

a. Dependent variables: Know 1: Insurance is free (0/1), Know 2: No regular fee (0/1), Know 3: No pay, no coverage (0/1), Know 4: Inform insurer (0/1), Know 5: Money back (0/1), Know 6: Life insurance (0/1) Know 7: PhilHealth Availability (0/1), Know 8: PhilHealth Coverage (0/1) Know 9: Frequency Payment (0/1), Know 10: Claim (0/1), Know 11: Premium (0/1), Know 12: Typhoon (0/1)

b. All regressions control for gender of respondent, age and schooling of head, number of children, type of income source.

c. Standard errors in parentheses are clustered at the barangay level.

d. Items marked with a star are not included in the Literacy index as they allow for ambiguous answers.

Table 2.13: Linear Regression - Impact of Brochure and Spill-over on Knowledge-Items (Female Sample)

	Know 1	Know 2	Know 3*	Know 4*	Know 5	Know 6	Know 7*	Know 8	Know 9	Know 10	Know 11	Know 12
Brochure	-0.01	-0.01	-0.00	-0.06***	-0.01	0.04	-0.02	0.03	0.02	0.00	-0.00	-0.01
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.04)	(0.04)	(0.03)
Spill-over	-0.02	-0.02	0.07**	-0.04**	-0.07*	-0.01	-0.01	0.01	-0.02	-0.06	-0.04	-0.06
	(0.03)	(0.04)	(0.03)	(0.02)	(0.04)	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)
Observations	1884	1884	1876	1881	1884	1884	1879	1884	1884	1884	1884	1884

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

a. Dependent variables: Know 1: Insurance is free (0/1), Know 2: No regular fee (0/1), Know 3: No pay, no coverage (0/1), Know 4: Inform insurer (0/1), Know 5: Money back (0/1), Know 6: Life insurance (0/1) Know 7: PhilHealth Availability (0/1), Know 8: PhilHealth Coverage (0/1) Know 9: Frequency Payment (0/1), Know 10: Claim (0/1), Know 11: Premium (0/1), Know 12: Typhoon (0/1)

b. All regressions control for gender of respondent, age and schooling of head, number of children, type of income source.

c. Standard errors in parentheses are clustered at the barangay level.

d. Items marked with a star are not included in the Literacy index as they allow for ambiguous answers.

Table 2.14: Attitude at Delivery

	Number of Observations				Mean Score						
	All recipients	Follow-up recipients	Lost recipients	Control II	All recipients (1)	Follow-up recipients (2)	Lost recipients (3)	Control II (4)	p-value of t-test (1)-(4)	(2)-(4)	(3)-(4)
positive	1798	1287	511	460	1.2	1.2	1.1	1.2	.27	.4	.19
valuable	1795	1285	510	459	1.7	1.7	1.7	1.7	.98	.7	.5
simple	1779	1274	505	456	1.1	1.2	1.1	1.2	.58	.73	.41
Attitude before	1778	1273	505	455	4	4.1	3.9	4.1	.42	.64	.21

Note: This table compares attitude at the time of brochure distribution for all brochure recipients, recipients that were followed up, and recipients that were not followed up. It also includes the results of control group II for the respective attitude items.
Attitude before is the sum of scores on the items positive-valuable-simple

Table 2.15: Attitude at Delivery (Female sample)

	Number of Observations		Mean Score		
	Follow-up recipients	Control II	Follow-up recipients (1)	Control II (2)	p-value of t-test (1)-(2)
positive	868	349	1.14	1.23	.21
valuable	867	348	1.71	1.74	.43
simple	861	346	1.1	1.19	.25
Attitude before	860	346	3.96	4.16	.12

Note: This table compares attitude at the time of the delivery for female brochure recipients with female Control II respondents. The attrition sample is omitted since gender is only available in the .
Attitude before is the sum of scores on the items positive-valuable-simple

Table 2.16: Attitude - by Treatment Group

	Campaign villages							
	All (T=1) (1)	Brochure HH (TT=1) (2)	Control I (TT=0) (3)	Control II (T=0) (4)	p-value of (2)-(3)	(2)-(4)	(3)-(4)	(1)-(4)
1 positive	1.41	1.39	1.46	1.23	0.13	0.01	0.00	0.00
Std error	0.02	0.03	0.04	0.06				
2 rich	-0.24	-0.22	-0.28	-0.20	0.28	0.72	0.24	0.51
Std error	0.03	0.03	0.05	0.05				
3 fair	1.28	1.26	1.33	1.14	0.15	0.05	0.00	0.01
Std error	0.02	0.03	0.04	0.05				
4 reliable	1.54	1.53	1.57	1.35	0.35	0.00	0.00	0.00
Std error	0.02	0.02	0.03	0.05				
5 necessary	1.54	1.51	1.62	1.57	0.02	0.23	0.41	0.52
Std error	0.02	0.03	0.04	0.04				
6 affordable	-0.08	-0.05	-0.15	0.11	0.16	0.04	0.01	0.01
Std error	0.03	0.04	0.06	0.07				
7 expensive	-0.43	-0.40	-0.52	-0.25	0.07	0.05	0.00	0.01
Std error	0.03	0.04	0.06	0.06				
8 valuable	1.74	1.74	1.75	1.72	0.73	0.65	0.52	0.57
Std error	0.02	0.02	0.03	0.04				
9 simple	1.16	1.14	1.19	1.17	0.39	0.64	0.79	0.80
Std error	0.03	0.03	0.05	0.06				
10 powerful	0.72	0.72	0.71	0.65	0.77	0.25	0.46	0.27
Std error	0.03	0.03	0.05	0.05				
11 safe	1.28	1.26	1.32	1.23	0.21	0.63	0.15	0.41
Std error	0.02	0.03	0.04	0.05				
12 beneficial	1.56	1.55	1.58	1.42	0.48	0.01	0.01	0.00
Std error	0.02	0.02	0.03	0.05				
13 easy	0.19	0.23	0.08	0.32	0.06	0.29	0.02	0.11
Std error	0.03	0.04	0.07	0.07				
14 close	-0.24	-0.25	-0.19	-0.08	0.44	0.03	0.24	0.05
Std error	0.03	0.04	0.06	0.07				

Continued on next page...

... table 2.16 continued

	Campaign villages							
	All (T=1) (1)	Brochure HH (TT=1) (2)	Control I (TT=0) (3)	Control II (T=0) (4)	(2)-(3)	*p*-value of (2)-(4)	(3)-(4)	(1)-(4)
15 good	1.73	1.72	1.74	1.55	0.44	0.00	0.00	0.00
Std error	0.01	0.02	0.03	0.04				
16 wise	1.24	1.21	1.34	0.96	0.00	0.00	0.00	0.00
Std error	0.02	0.02	0.04	0.05				
17 useful	1.76	1.77	1.74	1.61	0.31	0.00	0.01	0.00
Std error	0.01	0.02	0.03	0.04				
18 self-determined	0.01	0.04	-0.08	-0.05	0.00	0.06	0.56	0.22
Std error	0.02	0.02	0.03	0.04				
19 healthy	0.70	0.70	0.70	0.92	0.96	0.00	0.00	0.00
Std error	0.03	0.03	0.05	0.05				
N	2099	1509	590	448				

Table 2.17: Factor-loadings for "Insurance in General"-items (original and modified loadings)

	Factor 1	*Factor 2*	*Factor 3*	*Factor 1*	*Factor 2*	*Factor 3*
positive	0.07	**0.62**	-0.15	0	**0.59**	0
rich	-0.38	**0.53**	-0.03	0	**0.60**	0
fair	**0.61**	-0.01	0.08	**0.58**	0	0
reliable	**0.73**	0.07	-0.03	**0.67**	0	0
necessary	**0.63**	0.21	-0.03	**0.63**	0	0
affordable	-0.07	0.19	**0.79**	0	0	**0.79**
cheap	0.23	-0.22	**0.76**	0	0	**0.62**
valuable	0.37	**0.55**	-0.11	0	**0.44**	0
simple	0.08	**0.53**	0.29	0	**0.42**	0
powerful	0.15	**0.59**	0.16	0	**0.49**	0
safe	**0.67**	-0.01	0.23	**0.67**	0	0
beneficial	**0.66**	0.05	0.13	**0.65**	0	0

Table 2.18: Factor-loadings for "Buying Health Insurance" (original and modified loadings)

	Factor 1	*Factor 2*	*Factor 1*	*Factor 2*
easy	0.19	**0.53**	0	**0.62**
close	0.01	**0.77**	0	**0.76**
good	**0.79**	0.05	**0.77**	0
wise	**0.69**	0.00	**0.65**	0
useful	**0.77**	0.07	**0.76**	0

Table 2.19: Linear Regression - Dependent Variable modified Factors for Insurance in general

	Insurance in General			Buying Health Insurance	
	Factor 1	Factor 2	Factor 3	Factor 1	Factor 2
Brochure received	0.14	0.04	-0.13**	0.35**	-0.13**
	(0.10)	(0.08)	(0.06)	(0.16)	(0.06)
Spill-over	0.22**	0.08	-0.21***	0.37**	-0.15**
	(0.10)	(0.07)	(0.06)	(0.16)	(0.07)
Observations	2469	2469	2469	2514	2514

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses are clustered at the barangay level.

OLS with modified factors as dependent variables

For "Insurance in General" Factor 1 captures fair - reliable - necessary - safe - beneficial

Factor 2 captures positive - rich - valuable - simple - powerful

Factor 3 captures affordable - cheap

For "Buying Health Insurance" Factor 1 captures good - wise - useful

Factor 2 captures easy - close

Table 2.20: Linear Regression - Impact of Brochure and Spill-over on Attitude-Items

	Factor 1: Security Issues					Factor 2: Diffuse Attitude					Factor 3: Financial Issues	
	fair	reliable	necessary	save	beneficial	positive	rich	valuable	simple	powerful	affordable	cheap
Brochure received	0.12	0.19**	-0.06	0.01	0.13	0.15**	-0.03	0.02	-0.04	0.08	-0.12	-0.12*
	(0.09)	(0.09)	(0.06)	(0.11)	(0.10)	(0.07)	(0.06)	(0.06)	(0.09)	(0.08)	(0.08)	(0.06)
Spill-over	0.17*	0.22**	0.05	0.07	0.15	0.23***	-0.07	0.03	0.01	0.07	-0.20**	-0.23***
	(0.10)	(0.09)	(0.06)	(0.11)	(0.11)	(0.07)	(0.07)	(0.06)	(0.10)	(0.09)	(0.09)	(0.08)
Observations	2542	2545	2543	2528	2536	2543	2545	2538	2530	2530	2543	2541

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All regressions control for gender of respondent, age and schooling of head, number of children, type of income source.

Standard errors in parentheses are clustered at the barangay level.

Table 2.21: Linear Regression - Impact of Brochure and Spill-over on Attitude-Items for Buying Health Insurance

	Factor 1: Worthiness			Factor 2: Accessibility	
	good	wise	useful	easy	close
Brochure received	0.18*	0.24**	0.17**	-0.04	-0.18**
	(0.10)	(0.12)	(0.08)	(0.10)	(0.08)
Spill-over	0.19*	0.37***	0.13	-0.17	-0.10
	(0.10)	(0.12)	(0.09)	(0.12)	(0.09)
Observations	2542	2531	2534	2542	2541

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses are clustered at the barangay level.

All regressions control for gender of respondent,

age and schooling of head, number of children, type of income source.

Table 2.22: Take-up of PhilHealth Insurance in Experimental Villages

	Campaign villages	Interview villages	Control villages
Total	353	280	755
IPP	211	166	391
SP	142	114	256
Number bgy	34	26	197

Table 2.23: Take-up of PhilHealth Insurance by Type of Village

	Campaign and Interview (1)	Campaign-villages (2)	Interview-villages (3)	Control-villages (4)	P-value of (1)-(4)	(2)-(4)	(3)-(4)
Population	2295	2276	2321	2262	.871	.956	.844
Std error	160.53	190.18	279.04	108.09			
Total take-up	10.55	10.38	10.77	4.31	0	0	0
Std error	1.12	1.43	1.82	.49			
Total take-up rate	.005	.005	.005	.002	0	0	0
Std error	0	0	0	0			
IPP take-up	6.28	6.21	6.38	2.23	0	0	0
Std error	.72	1.08	.91	.38			
IPP take-up rate	.003	.003	.003	.001	0	0	0
Std error	0	0	0	0			
SP take-up	4.27	4.18	4.38	1.46	0	.001	.002
Std error	.83	.99	1.43	.29			
SP take-up rate	.002	.002	.002	.001	.011	.029	.101
Std error	0	0	0	0			
Observations	60	34	26	175			

Population data from www.nscb.gov.ph .

Table 2.24: Take-up of PhilHealth - Brochure Recipients and Different Control Groups

	Campaign villages							
	All (T=1) (1)	Brochure HH (TT=1) (2)	Control I (TT=0) (3)	Control II (T=0) (4)	P-value of (2)-(3)	(2)-(4)	(3)-(4)	(1)-(4)
Take-up (0/1)	0.01	0.01	0.01	0.04	0.81	0.06	0.10	0.03
Std error	0.00	0.01	0.01	0.02				
Take-up IPP (0/1)	0.01	0.01	0.01	0.02	0.51	0.67	0.36	0.52
Std error	0.00	0.01	0.01	0.01				
Take-up SP (0/1)	0.00	0.00	0.01	0.03	0.51	0.01	0.16	0.01
Std error	0.00	0.00	0.01	0.01				
Observations	593	422	171	118				

Sample consists only of respondents not enrolled with PhilHealth at follow-up survey

Table 2.25: Regression models - Dependent Variable: Total take-up

	(1) LPM	(2) LPM	(3) LPM	(4) LPM	(5) Logit	(6) Logit	(7) Logit	(8) Logit
main								
Brochure (TT=1 w/o T=0) (d)	0.003	0.003			0.003	0.002		
	(0.01)	(0.01)			(0.01)	(0.01)		
Age head		0.000		0.000		0.000		0.000
		(0.00)		(0.00)		(0.00)		(0.00)
Female respondent (d)		0.008		0.008		0.005		0.007
		(0.01)		(0.01)		(0.00)		(0.01)
Schooling head		0.002		0.002		0.001		0.002
		(0.00)		(0.00)		(0.00)		(0.00)
Trade (d)		-0.000		-0.005		-0.000		-0.004
		(0.01)		(0.01)		(0.01)		(0.01)
Shock history (d)		0.034*		0.021		0.027		0.018
		(0.02)		(0.02)		(0.02)		(0.02)
Brochure (T=1 w/o TT=0) (d)			-0.028	-0.029			-0.028	-0.025
			(0.02)	(0.02)			(0.02)	(0.02)
Observations	592	584	539	533	592	584	539	533

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Standard errors clustered at village-level in parentheses.

Table 2.26: Regression models - Dependent Variable: IPP take-up

	(1) LPM	(2) LPM	(3) LPM	(4) LPM	(5) Logit	(6) Logit	(7) Logit	(8) Logit
main								
Brochure (TT=1 w/o T=0) (d)	0.006	0.006			0.006	0.001		
	(0.01)	(0.01)			(0.01)	(0.00)		
Age head		0.001		0.001		0.000		0.000
		(0.00)		(0.00)		(0.00)		(0.00)
Female respondent (d)		0.003		0.007		0.001		0.004
		(0.01)		(0.01)		(0.00)		(0.00)
Schooling head		0.004		0.004		0.001		0.002**
		(0.00)		(0.00)		(0.00)		(0.00)
Trade (d)		0.002		-0.009		-0.000		-0.004
		(0.01)		(0.01)		(0.00)		(0.00)
Shock history (d)		0.029*		0.021		0.008		0.010
		(0.02)		(0.02)		(0.01)		(0.01)
Brochure (T=1 w/o TT=0) (d)			-0.005	-0.005			-0.005	-0.002
			(0.01)	(0.01)			(0.01)	(0.01)
Observations	592	584	539	533	592	584	539	533

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Standard errors clustered at village-level in parentheses.

Table 2.27: Regression models - Dependent Variable: SP take-up

	(1) LPM	(2) LPM	(3) LPM	(4) LPM	(5) Logit	(6) Logit	(7) Logit	(8) Logit
main								
Brochure (TT=1 w/o T=0) (d)	-0.003	-0.004			-0.003	-0.001		
	(0.01)	(0.01)			(0.01)	(0.00)		
Age head		-0.000		-0.000		-0.000		-0.000
		(0.00)		(0.00)		(0.00)		(0.00)
Female respondent (d)		0.005		0.001				0.000
		(0.00)		(0.01)				(0.00)
Schooling head		-0.001		-0.002		-0.000		-0.001
		(0.00)		(0.00)		(0.00)		(0.00)
Trade (d)		-0.003		0.003				0.002
		(0.00)		(0.01)				(0.01)
Shock history (d)		0.006		0.000		0.001		0.000
		(0.01)		(0.01)		(0.00)		(0.00)
Brochure (T=1 w/o TT=0) (d)			-0.023	-0.024			-0.023	-0.020
			(0.02)	(0.02)			(0.02)	(0.02)
Observations	592	584	539	533	592	340	539	533

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Standard errors clustered at village-level in parentheses.

Table 2.28: Random Effects Regression - Dependent Variable: Total take-up

	(1) LPM	(2) LPM	(3) LPM	(4) LPM	(5) Logit	(6) Logit	(7) Logit	(8) Logit
main								
Brochure (TT=1 w/o T=0) (d)	0.001	0.002			0.001	0.001		
	(0.01)	(0.01)			(0.01)	(0.00)		
Age head		0.000		0.001		0.000		0.000
		(0.00)		(0.00)		(0.00)		(0.00)
Schooling head		0.002		0.003		0.001		0.001
		(0.00)		(0.00)		(0.00)		(0.00)
Female respondent		0.008		0.009				
		(0.01)		(0.01)				
Trade		-0.001		-0.012				
		(0.01)		(0.01)				
Shock history (d)		0.034*		0.022		0.016		0.008
		(0.02)		(0.02)		(0.01)		(0.01)
Brochure (T=1 w/o TT=0) (d)			-0.044	-0.046			-0.021	-0.017
			(0.04)	(0.04)			(0.02)	(0.02)
Observations	592	584	539	533	592	589	539	535

Note: Estimations in (1)-(4) use heteroskedasticity-robust standard errors.
Columns (5)-(8) present marginal effects where the individual-specific effect is zero.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.29: Random Effects Regression - Dependent Variable: IPP take-up

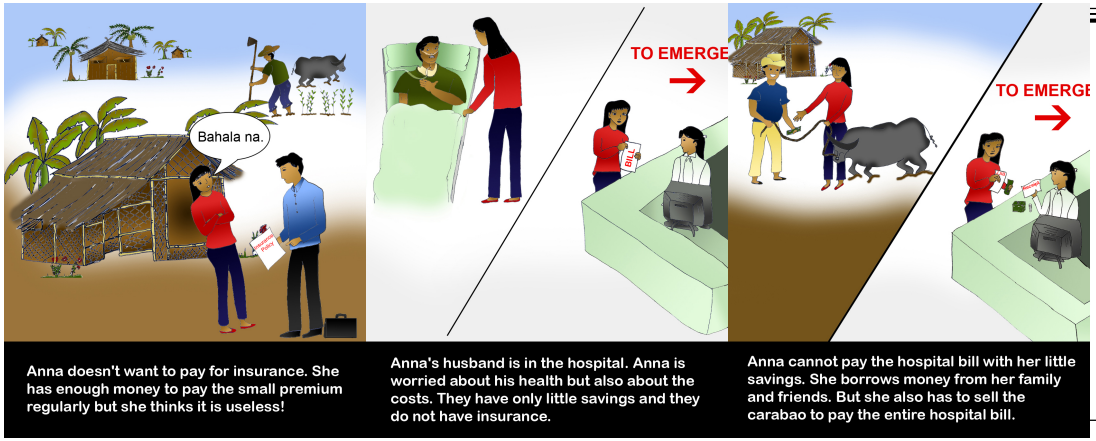
	(1) LPM	(2) LPM	(3) LPM	(4) LPM	(5) Logit	(6) Logit	(7) Logit	(8) Logit
main								
Brochure (TT=1 w/o T=0) (d)	0.005	0.006			0.004	0.000		
	(0.01)	(0.01)			(0.01)	(0.00)		
Age head		0.001		0.001		0.000		0.000
		(0.00)		(0.00)		(0.00)		(0.00)
Schooling head		0.004		0.004		0.000		0.002
		(0.00)		(0.00)		(0.00)		(0.00)
Female respondent		0.003		0.007				
		(0.01)		(0.01)				
Trade		0.002		-0.009				
		(0.01)		(0.01)				
Shock history (d)		0.028*		0.021		0.003		0.011
		(0.02)		(0.02)		(0.01)		(0.01)
Brochure (T=1 w/o TT=0) (d)			-0.005	-0.005			-0.004	-0.003
			(0.01)	(0.01)			(0.01)	(0.01)
Observations	592	584	539	533	592	589	539	535

Note: Estimations in (1)-(4) use heteroskedasticity-robust standard errors.
Columns (5)-(8) present marginal effects where the individual-specific effect is zero.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.30: Random Effects Regression - Dependent Variable: SP take-up

	(1) LPM	(2) LPM	(3) LPM	(4) LPM	(5) Logit	(6) Logit	(7) Logit	(8) Logit
main								
Brochure (TT=1 w/o T=0) (d)	-0.003	-0.004			-0.003	-0.001		
	(0.01)	(0.01)			(0.01)	(0.00)		
Age head		-0.000		-0.000		-0.000		-0.000
		(0.00)		(0.00)		(0.00)		(0.00)
Schooling head		-0.001		-0.001		-0.000		-0.000
		(0.00)		(0.00)		(0.00)		(0.00)
Shock history (d)		0.006		0.003		0.001		0.000
		(0.01)		(0.01)		(0.00)		(0.00)
Female respondent		0.005		0.001				
		(0.00)		(0.00)				
Brochure (T=1 w/o TT=0) (d)			-0.051	-0.051			-0.005	-0.005
			(0.04)	(0.05)			(0.01)	(0.01)
Observations	592	584	539	533	592	589	539	535

Note: Estimations in (1)-(4) use heteroskedasticity-robust standard errors.
Columns (5)-(8) present marginal effects where the individual-specific effect is zero.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$



Insurance ABC

An insurance can help you in emergency situations. If somebody has an accident of falls sick a health insurance can help you with the cost of the treatment. There are insurances that help you if your field is destroyed or your house burns down. Some insurances pay for the costs of the funeral if somebody dies. All these are insurable events.

Ask any insurance agent about available insuance policies. Read carefully all documents. Make sure you need the insurance and you understand the important details like:

- What event is covered?
- Who is covered?
- Who are the beneficiaries? This means, who receives the money especially from a life insurance?
- How much premium do you have to pay?
- When and where do you have to pay the premium?
- How to make a claim?
- What happens if you do not pay your premium on time or stop paying premium?

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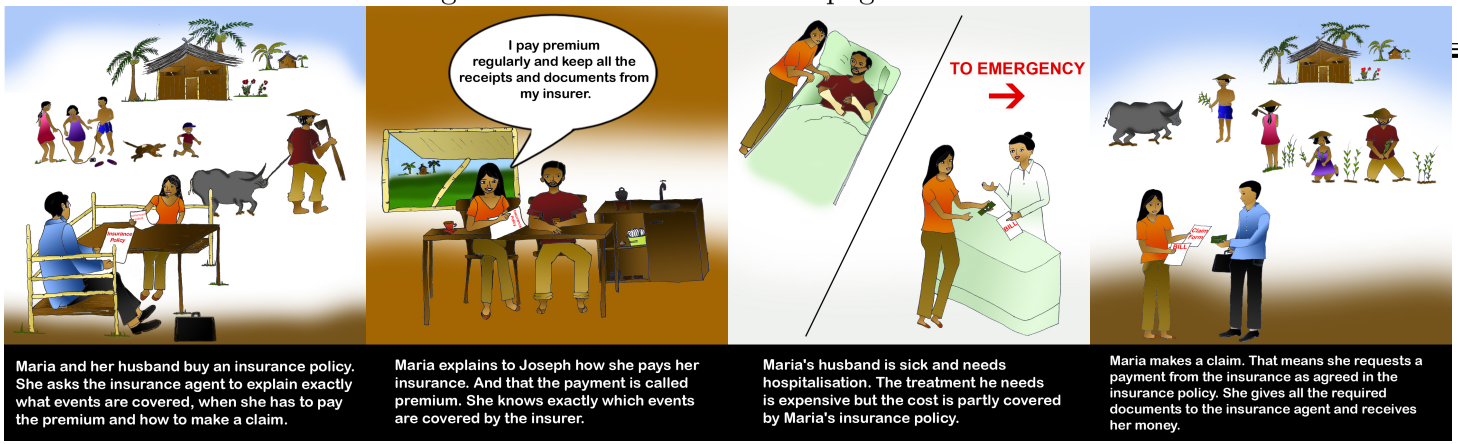
Figure 2.2: Brochure - Front page



Insurance ABC Glossary

An INSURED EVENT is an event whose occurrence leads to a payment/Recompensation from the insurance.
The person who receives this payment is called **BENEFICIARY**.

Different types of insurances cover different insured events:

LIFE INSURANCE – If a person dies, his/her beneficiary receives money.
FIRE INSURANCE – If a house burns down, the beneficiary receives money.
CROP INSURANCE – If the crop of an beneficiary gets destroyed, he receives money
HEALTH INSURANCE – If a person gets sick, he gets a discount on the hospital bill.

Even more types of insurances exist.
A **PREMIUM** is a payment the insured person has to pay regularly to the insurance.

With a **CLAIM FORM** the beneficiary requests a payment from the insurance after an insured event occurred.

Figure 2.3: Brochure - Back page

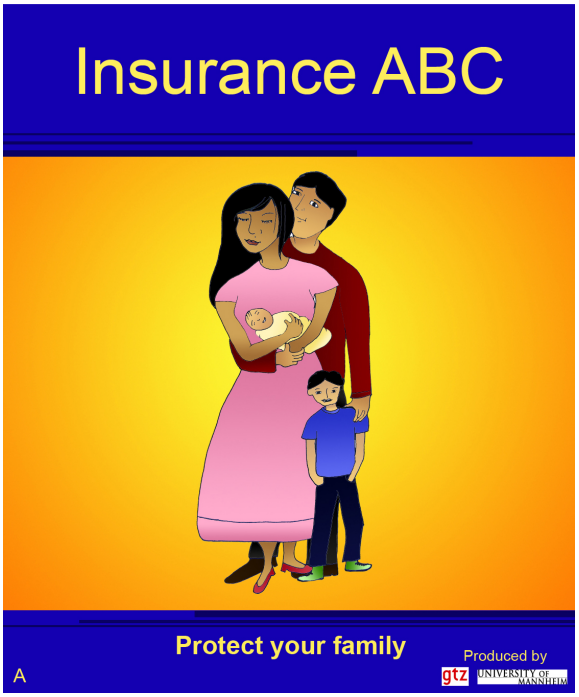


Figure 2.4: Cover Design - Protection

Figure 2.5: Cover Design: Trust

Figure 2.6: Cover Design - Optimism

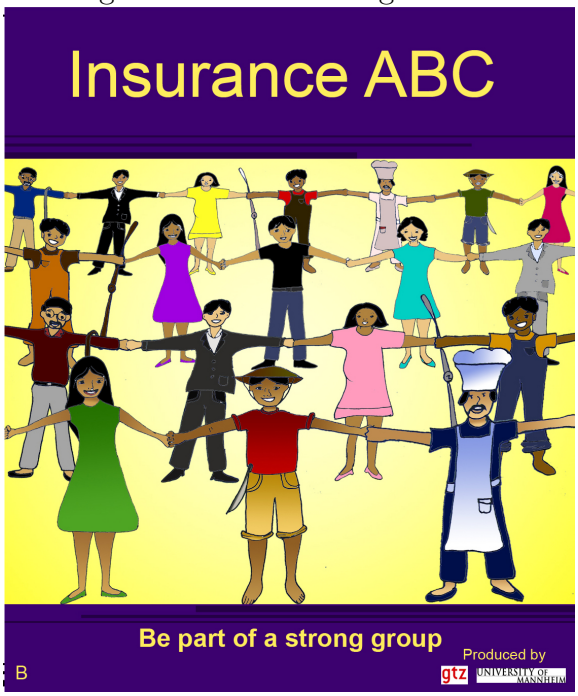


Figure 2.7: Cover Design: Solidarity

Figure 2.8: Questionnaire Items - Knowledge

4.1**?** Do you have any idea how an insurance works?
1 – not at all 2 – a bit 3 – Yes 98 – refuses to answer 99 – does not know

Read: I will ask you some questions to check your level of familiarity with insurances.. Please, tell me if this is true or false.

4.2. If you have an insurance it is free of any fees or payments for you.
0 – False 1 – True 98 – refuses to answer 99 – does not know

4.2.2 If you have an insurance you do not have to pay regularly, you can just pay when you want.
0 – False 1 – True 98 – refuses to answer 99 – does not know

4.2.3 If you have an insurance and you are late with your payments or do not pay, the insurance does not help you when you have a problem.
0 – False 1 – True 98 – refuses to answer 99 – does not know

4.2.4 If you have an insurance, you need to contact your insurer if something happens that is covered by the insurance.
0 – False 1 – True 98 – refuses to answer 99 – does not know

4.2.5 If you pay regularly to that insurance and you never need to use it, you get your money back.
0 – False 1 – True 98 – refuses to answer 99 – does not know

4.2.6**.** PhilHealth membership-cards are accepted in every hospital.
0 – False 1 – True 98 – refuses to answer 99 – does not know

4.2.7 PhilHealth covers all inpatient and outpatient services.
0 – False 1 – True 98 – refuses to answer 99 – does not know

Read: In your opinion, if you have an insurance you pay...
1 –...only once when you make the contract
2 –...regularly every month, every 3 months once a year
3 –...only when I have a problem that is covered by the insurance.
98 – refuses to answer 99 – does not know

4.3.2 What is a claim?
1 –The contract with an insurance.
2 – The regular payment an insured person makes to the insurance company
3 The formal way to get money from the insurer for an insured event.
98 – refuses to answer 99 – does not know

4.3. What is an insurance premium?
1 –The interest rate you will get for my payments.
2 –The regular payment an insured person makes to the insurance company
3 –Person who sells insurance
98 – refuses to answer 99 – does not know

4.4.1 Imagine a man wants to protect his family against his death, can he buy an insurance that gives money to his family after his death?
0 – no 1 – yes 98 – refuses to answer 99 – does not know

4.4.2 Farmers in the Philippines often run the risk that a typhoon destroys their crops. What is in your opinion the best way to deal with that?
1 – Making donation to church.
2 – Saving money for the case that his field is destroyed.
3 – Buying a crop insurance, which means signing a contract and regularly paying premium.
98 – refuses to answer 99 – does not know

Figure 2.9: Questionnaire Items - Attitude

We are trying to find out how you feel about insurance. Please tell us how you feel and rate your feelings toward Insurance. There are no wrong or correct answers. Just let us know your first impression.
As an example rate the word TAIPHOON: strong – weak , good – bad, slow – fast

3.11. How do you feel towards insurance in general:

1	Positive	quite positive	Neutral	quite negative	Negative
	-2	-1	0	1	2
2	Rich	quite rich	Neutral	quite poor	Poor
	-2	-1	0	1	2
3	Unfair	quite unfair	Neutral	quite fair	Fair
	-2	-1	0	1	2
4	Unreliable	quite unreliable	Neutral	quite reliable	Reliable
	-2	-1	0	1	2
5	Necessary	quite necessary	Neutral	quite unnecessary	Unnecessary
	-2	-1	0	1	2
6	Affordable	quite affordable	Neutral	quite unaffordable	Unaffordable
	-2	-1	0	1	2
7	Expensive	quite expensive	Neutral	quite cheap	Cheap
	-2	-1	0	1	2
8	Valuable	quite valuable	Neutral	quite worthless	Worthless
	-2	-1	0	1	2
9	Simple	quite simple	Neutral	quite complicated	Complicated
	-2	-1	0	1	2
10	Powerful	quite powerful	Neutral	quite powerless	Powerless
	-2	-1	0	1	2
11	Risky	quite risky	Neutral	quite save	save
	-2	-1	0	1	2
12	Wasteful	quite wasteful	Neutral	quite beneficial	Beneficial
	-2	-1	0	1	2

3.12. How do you feel towards buying health insurance:

1	Difficult	quite difficult	Neutral	quite easy	Easy
	-2	-1	0	1	2
2	Close	quite close	Neutral	quite far	Far
	-2	-1	0	1	2
3	Bad	quite bad	Neutral	quite good	Good
	-2	-1	0	1	2
4	Foolish	quite foolish	Neutral	quite wise	Wise
	-2	-1	0	1	2
5	Useless	quite useless	Neutral	quite useful	Useful
	-2	-1	0	1	2
7	Healthy	quite healthy	Neutral	quite sick	Sick
	-2	-1	0	1	2

CHAPTER III

Strengthening Rural Producer Organizations in Uganda: The impact on household labor decisions

3.1 Introduction

In rural Sub-Saharan Africa, producer organizations (PO) promise improved market access for smallholder farmers. It is under the rationale that collective action offsets problems related to diseconomies of scale that farmers join in marketing and other activities (Heyer et al., 1999; Hill et al., 2008; The World Bank, 2008). This way of eradicating market imperfections that the individual farmers encounter aims at improving outcomes, most importantly, at cutting costs and increasing profits. Less clear is the effect that producer organizations (POs) have on the allocation of inputs. Market imperfections are likely to cause inefficient use of production factors. It is at the heart of this study to test whether POs affect the most flexible input factor that smallholder farmers possess: labor.

Generally, POs are member driven organizations. Their success depends on the members' participation in joint activities. But despite the potential benefits of collective action, reality shows that member participation is low and farmers continue to engage in markets individually (Fafchamps and Hill, 2005; Hill, 2010). Two issues can, at least partly, be blamed for the members' reluctance to avail of the services

offered by their PO, leading them to accept the adverse market conditions they are facing when making individual sales. Firstly, farmers struggle with cash constraints that render participation in PO marketing activities uninteresting as they cause substantial delays in payment as compared to sales to itinerant traders. Secondly, the POs sales processes are not transparent for farmers. This creates distrust towards the organization and its leaders or the representatives of the umbrella organizations. Farmers fear that they will not be paid at all, or that the increases in prices that occur through bulking will not be shared completely.

Given these frictions PO members do not consider participation a valuable option to improve their market situation. Non-participation in the joint activities and continued inefficiencies are the consequence. These inefficiencies are likely to affect also labor allocation (Schmitz and Nadvi, 1999).

The study at hand tries to assess whether addressing the above mentioned weaknesses of POs effectively changes the allocation of labor. This is based on the assumption that improved market access affects input allocation (Sadoulet et al., 1998), a hypotheses that remains to be verified experimentally. Schmitz and Nadvi (1999) point out that rendering markets accessible can result in the mobilization of untapped labor resources - a necessity for the development process. To test this assumption, we use a randomized controlled trial in Uganda to assesses how changes in the procedures of the PO affect the labor input decisions of their members. We randomly allocate three different alterations in the PO procedures to a set of 167 POs.

The first alteration tackles the problem of delayed payments concurrent to bulking activities. It effectively reduces the period of time that PO members have to wait before receiving the revenue from sales through the PO. By reducing the postponement in payment, inefficiencies for cash constrained farmers are reduced. This

could affect the demand for hired labor as it increases the capacity to pay laborers immediately after harvesting. Adams (1991) stresses the importance of smallholder agriculture for the demand of agricultural wage labour for the case of Zimbabwe. Other than in developed countries, in rural African labour markets daily payment or payment on piece/task basis is prevalent for rural laborers. Sender (2005) studies the labour market in Mozambique and reports that 70 percent of agricultural wage laborers receive either daily pay or are paid by the piece delivered or task accomplished. Under such conditions and under the assumption that a credit constraint exists, smallholder farmers with cash constraints are unattractive employers. Naturally, laborers prefer to offer their labor to smallholder farmers who do not engage in bulking and who remunerate without postponement. Simultaneously PO members might refrain from hiring additional labor due to their inherent incapacity to pay wages promptly after harvesting.

The second intervention tackles the problem of opaque sales procedures. Ordinary PO members are not present at the time of the final sale of their bulked produce. Hence, their engagement in joint marketing requires trust in the PO in general and in its leaders more specifically. If trust is low the inclination to participate is low, too. By providing information on the final sale this intervention tries to offset the lack of trust. In a first step, a voucher system is put in place to document the quantity a farmer delivers to the PO. To reinforce voucher distribution, financial incentives are given to those who issue the vouchers. In a second step, external consultants are present when the PO leaders make the final sale. These consultants inform the smallholder farmers via sms text messages about the details of the sale.

Both intervention can also have an effect on household labor input. Through participation in the bulking bargaining power increases which optimally results in

higher sales prices and the expectation of increased prices might affect labor input decisions.

We observe that PO members in Uganda adapt their household labor allocation and their demand for hired labor in the advent of these interventions. Mere anticipation of changes that promise better market participation through the PO suffices to induce these effects. Furthermore, the distinct treatment arms prove to have differential effects on the type of labor that is mobilized. Information on sales procedures rather increases the amount of household labor, while relieving cash constraints at harvest increases the labor hired for crop production. This work adds to the stock of knowledge on rural POs, by assessing the effects on labor allocation, and by assessing the role trust plays for this.

3.2 Context and the Interventions

3.2.1 Labor Market Context

Uganda's population has increased from around 9 million in the 1969 to an estimated 34 million today[1]. Close to 90 percent of the population reside in rural areas. Naturally, the extent of population growth poses stress for food security and employment. Official estimations show that 66 percent of the labor force make a living in agriculture (Uganda Bureau of Statistics 2010) [2]. In rural areas the share of people aged 15-60 who work in agriculture is above 70 percent. As in many African countries, Uganda's agriculture relies heavily on smallholder farming. Due to unfavorable market structures smallholders seek refuge in the formation of POs to improve their market situation.

[1] See `http://countrystat.org/home.aspx?c=UGA&tr=12`

[2] See `http:\www.ubos.org`

Heyer et al. (1999) point out that such group formation tackles market imperfections by performing efficiency functions. These are the main drivers for farmers to engage in collective action as the group activity promises to improve their situation vis-a-vis adverse market conditions, e.g. high transaction costs and information asymmetries.

The World Development Report 2008 (The World Bank, 2008) recognizes the linkages between improved market access for smallholder farmers and increased employment in agriculture. It emphasizes further the role for poverty reduction in rural areas through increased employment opportunities. A rigorous assessment of such claims needs to be provided yet.

3.2.2 Producer Organization Context

Develtere et al. (2008) explain the history of producer organizations in Uganda that started as early as 1913. Later, after independence in 1962 the Ugandan government heavily regulated the market for agricultural outputs and organized marketing boards. Such boards set constant buying prices independently of the time of the season, liberating the farmer of the question when to sell (Ponte, 2002; Shepherd, 2011). The question to whom to sell was equally redundant at that time, since producer organizations were the only marketing option for small holders (Hill et al., 2008; Ponte, 2002).

With liberalization starting in the 1990s the POs saw change internally as well as in their environment. Under the structural adjustment programs liberalization in the coffee market was promoted and with the introduction of competitive market structures the cooperatives nearly disappeared (Ponte, 2002). Develtere et al. (2008, p. 156) document that many POs collapsed due to "...massive corruption, mismanage-

ment, theft, failure to hold elections, failure to surrender members' deposits, failure to hold elections on time, favoritism and dismissal of staff, refusal of officials to vacate office after being duly voted out...". Against this historical background, the role of trust in producer organization gains in relevance.

3.2.3 Interventions

We designed two interventions to tackle barriers that keep PO members from participating in the bulk sales: partial payment at the harvest delivery and increase in transparency of the sales process. We are not aware of previous evidence in the literature documenting the effectiveness of these measures on labor allocation.

Cash on Delivery

This intervention provides randomly selected POs with the financial means to pay members a share of their revenue right when the farmer delivers the harvest to the PO. The intervention started by distributing vouchers to PO members. Holding such a voucher qualified the farmer to receive 30 percent of the total sales price right at delivery. The remaining 70 percent of the sales price were to be paid out after the PO made the final sale.

Information on Sales

POs randomly allocated to receive this intervention set up a voucher system that documents the quantities a farmer delivered to the PO. At delivery the farmer and the PO representative would fill out a slip stating the quantity that the farmer delivered. The PO representatives were given financial incentives to distribute these vouchers. All PO members were informed about this documentation system and about the incentive system for the representatives.

In addition to installing and reinforcing the voucher system, an external consultant was hired that provided the ordinary members with information on the final sale. The ordinary members elected two of them who would receive a text message from the consultant right after the final sale. The text message stated the final quantity and the price per kilogram. The two elected members received training on how to interpret and explain the text message. The regular members received training on how to interpret the information from that text message.

Both interventions

In POs selected for both interventions the members received training on using the cash-on-delivery vouchers, the delivery slips and in interpreting the information-on-sales text messages.

3.3 Experimental Design and Data Collection

For the experiment we applied a cluster randomized trial where the unit of randomization is the PO and the unit of analysis is the individual PO member. From our sample of 167 POs 42 were selected to receive the cash-on-delivery intervention only, 42 were selected to receive the information-on-sales intervention only, 40 POs were allocated to receive both the cash-on-delivery and the information-on-sales interventions, and 43 were allocated to the control group.

The organizational structure these member driven organizations is such that on the district level POs are grouped in depot committees (DC). The DCs process and sell the crop[3]. Affiliation with a DC could potentially lead to imbalances in various

[3]The next highest level above the DC is the service organization. Each DC in our sample is member in one of the following five service organizations: the National Union of Coffee Agribusinesses and Farm Enterprises (NUCAFE), NKG Coffee Alliance Trust, Volunteer Effort for Development Concerns (VEDCO), the Uganda Co-operative Alliance (UCA) and Kulika.

observable and unobservable ways. To avoid such imbalances we stratified our sample of POs by DC. Stratification on the basis of variables that are likely to influence main outcomes also increases the power of the statistical analyzes (Bruhn and McKenzie, 2009). We stratified the POs according to DC affiliation into ten strata. The number of POs per DC/strata varies substantially with the smallest DC contributing seven and the largest one 30 POs. Furthermore, the smallest DC is exceptional as it does not have a PO allocated to receive the cash-on-delivery treatment only.

We conducted a baseline and an endline household survey, and interviewed at least two members of each PO, amounting to around 80 respondents per group. The interventions were implemented between November 2010 and September 2011. The endline data was collected in October 2011. For the analysis, we dropped three households from this sample because they had extreme values on a few characteristics[4].

3.3.1 Baseline Information by Treatment Status

Table 3.1 presents summary statistics at baseline for the sample by treatment status. The last columns list the p-value from t-tests to indicate whether any observed difference in means between the treatment group and the control group is statistically significant. Despite the randomization into treatment we find a handful of variables that are significantly different at the ten per cent level. At baseline, the PO members in our sample have around 7 household members, the head is on average roughly 50 years old, the spouse is about ten years younger, and in only 11 to 15 percent of the cases, depending on the group, is the head of the household

[4]One of the dropped households has 25,000 coffee trees as compared to the average in the sample of 660. The other two were remotely distant from the next producer market with over 45km as compared to the average distance of five km.

a woman. The average schooling of the household head lies between six and seven years and is lowest in the control group.

The only variable that we find to be significantly different at the five percent level is the number of parcels, with 2.5 parcels in the control group and 2.1 in the group that received both interventions. Roughly 50 percent of the treatment sample plants coffee, whereby only 43 percent of the control group do so. Yet, this difference is not significant. Around 34 to 39 percent of the farmers have more than 500 coffee plants. The share of members living more than five km away from the next agricultural market is around 30 per cent in the CoD group and in the group that received both interventions, but only for the former is the difference with the control group statistically significant at the 10 percent level. In the control group we find that 45 percent of the respondents live less than five km from the next market.

Availability of electricity in the household is low at 6 to 11 percent, compared to ownership of a mobile telephone which is at roughly 60 per cent. Ownership of savings accounts is at around 23 to 36 percent and about 33 to 41 per cent of the respondents hold informal savings. Trust is relatively high given that 75 percent trust their neighbor and over 50 per cent trust people in general. We create a proxy variable to measure trust in PO leaders which reveal quite high confidence in the leaders.

Table 3.1: Summary Statistics of Household Characteristics at Baseline

	Number of Observations				Mean of Variable				p-value of t-test with control group		
Variable Names	CoD	IoS	Both	Control	CoD	IoS	Both	Control	CoD	IoS	Both
Household size	82	85	77	75	7.24	7.2	7.09	7.25	.98	.9	.72
Age head	80	85	77	75	49.75	48.93	53.43	49.11	.78	.93	.07
Age spouse	69	74	63	60	39.99	39.42	41.98	38.23	.34	.53	.07
Gender head	82	85	77	75	.15	.11	.14	.13	.82	.59	.87
Yrs schooling head	82	84	77	74	6.6	7.17	6.21	6.04	.35	.06	.77
Yrs schooling spouse	69	74	63	60	4.79	5.68	5.49	4.85	.92	.15	.32
Distance to market>5km	82	85	77	75	.3	.46	.32	.45	.06	.94	.11
Savings account bank	82	84	77	75	.2	.27	.09	.19	.89	.2	.09
Savings account MFI	82	85	76	75	.23	.36	.29	.28	.49	.26	.9
Informal saving	82	85	77	75	.41	.31	.26	.33	.3	.71	.32
Owns mobile phone	82	85	77	75	.55	.69	.6	.63	.33	.37	.71
HH has electricity	82	85	77	75	.09	.11	.06	.11	.65	.99	.36
Head born in village	82	85	77	75	.65	.68	.7	.68	.66	.97	.78
Land size >7 acres	82	85	77	75	.44	.47	.34	.47	.73	.96	.11
Number of parcels	82	85	77	75	2.41	2.22	2.06	2.48	.75	.2	.03
Plants coffee	82	85	77	75	.48	.54	.45	.43	.54	.15	.73
>500 trees	82	85	77	75	.34	.39	.38	.36	.81	.71	.83
Received PO loan	79	82	77	72	.28	.3	.32	.36	.28	.46	.64
Neighbors can be trusted	82	84	77	75	.74	.73	.75	.77	.67	.5	.77
Most people can be trusted	82	84	77	75	.52	.54	.56	.59	.44	.52	.73
Trust PO leaders (proxy)	82	85	77	75	.72	.79	.74	.68	.59	.12	.42

Note: *Trust PO leaders (proxy)* is a proxy variable created from a question that asked the respondents to rate the amount of time PO leaders would spend on a hypothetical community project. If the respondent esteemed that the PO leaders would spend more time on the project than ordinary PO members the proxy takes on value one.

3.4 Results

3.4.1 Mean Comparison of Outcome Variables by Treatment Status

We present in tables 3.2 and 3.3 summary statistics of the four response variables that measure labor input: household member labor days in farming activities, household member labor days in crop production, hired labor days used on farm, and hired labor days for crop production. Table 3.2 presents the results for the first agricultural season, and table 3.3 those for the second season. We compute t-tests at baseline and at the endline and present the p-values in the last three columns.

Table 3.2: Summary Statistics of Labor Input: First Season

Variable Names	Number of Observations				Mean of Variable				p-value of t-test with control group		
	CoD	IoS	Both	Control	CoD	IoS	Both	Control	CoD	IoS	Both
Farming activities											
HH member labor days											
Baseline	82	85	77	75	238.91	222.24	202.09	204.04	.62	.59	.95
Endline	81	85	76	75	199.26	251.31	200.46	195.33	.9	.09	.86
Hired labor days											
Baseline	82	84	77	75	40.02	44.95	32.79	33.95	.64	.43	.93
Endline	80	85	77	75	70.39	71.22	40.56	50.57	.33	.24	.51
Crop production											
HH member labor days											
Baseline	82	84	75	75	58.51	64.42	61.47	64.59	.5	.99	.77

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Table 3.2: Summary Statistics of Labor Input: First Season

	Number of Observations				Mean of Variable				p-value of t-test with control group		
Variable Names	CoD	IoS	Both	Control	CoD	IoS	Both	Control	CoD	IoS	Both
Endline	81	85	76	75	81	104.55	84.62	76.24	.75	.07	.55
Hired labor days											
Baseline	47	49	38	42	35.7	27.8	25.32	27	.54	.93	.89
Endline	48	56	39	42	67.13	52.95	52.62	42.07	.15	.49	.6

Table 3.3: Summary Statistics of Labor Input: Second Season

	Number of Observations				Mean of Variable				p-value of t-test with control group		
Variable Names	CoD	IoS	Both	Control	CoD	IoS	Both	Control	CoD	IoS	Both
Farming activities											
HH member labor days											
Baseline	82	85	77	75	195.62	232.13	202.86	214.61	.56	.64	.74
Endline	81	85	76	74	227.95	241.85	250.57	193.38	.3	.15	.17
Hired labor days											
Baseline	82	84	77	75	39.24	48.43	35.08	32.84	.59	.27	.87
Endline	80	85	77	75	71.75	59.21	64	53.67	.43	.72	.7
Crop production											
HH member labor days											
Baseline	82	85	76	75	73.26	65.26	67.88	67.56	.6	.82	.98

Continued on next page...

Table 3.3: Summary Statistics of Labor Input: Second Season

	Number of Observations				Mean of Variable				p-value of t-test with control group		
Variable Names	CoD	IoS	Both	Control	CoD	IoS	Both	Control	CoD	IoS	Both
Endline	81	85	76	74	105.64	117.69	135.42	86.84	.32	.17	.06
Hired labor days											
Baseline	47	50	40	39	37.23	33.04	24.2	28.23	.48	.64	.67
Endline	44	50	41	46	91.02	56.48	94	58.17	.26	.92	.42

At baseline all outcomes are balanced, i.e. we do not find any significant difference neither in the first nor the second season between the means of the control group and the groups that received an intervention. For the first season, we find some evidence for impact as differences at the 10 per cent level exist at endline between the mean of the control group and that of the group that received information on sales. This is the case for household member labor days in farming activities, and for household member labor days in crop production. For the second season, we find evidence of a significant difference at the endline for the variable household member labor days in crop production for the group that received both interventions.

3.4.2 Estimation Strategy

Given that a few significant differences exist between the treated and the control group at baseline we complement the simple means comparison by OLS estimations. This allows controlling for the unbalanced items. We evaluate the impact of being a member in a PO that received one of the three interventions on our outcome variables: household member labor days in farming activities, household member labor days in crop production, hired labor days used on farm, and hired labor days for crop production, by estimating the following equation for member i:

$$Y_i = \alpha + \beta CoD_i + \gamma IoS_i + \delta Both_i + \eta X_i + \sum_{s=1}^{10} \lambda_s DC_{i,s} + \epsilon_i \tag{3.1}$$

where CoD_i is an indicator variable taking on value one if member i is from a PO that was assigned to distribute vouchers for cash-on-delivery, IoS_i is an indicator variable taking on value one if member i is from a PO where information-on-sales were distributed, and $Both_i$ is an indicator variable that denotes affiliation of member i to a PO that received both interventions at the same time. With coefficient β we

then estimate the Intention to treat (ITT) effect of the Cash on Delivery treatment, with γ the ITT effect of the information on sales treatment and with δ the ITT effect of implementing both interventions at the same time.

The vector X_i contains the control variables household size, years of schooling of the household head, age of the household head, a dummy for female household heads, a dummy for land size is larger than seven acres, the number of parcels, a dummy for farmer plants coffee, a dummy for number of coffee trees is above 500, a dummy for savings at a bank, a dummy for household has a mobile phone, a dummy for household has electricity, a dummy for distance to next producer market is larger than 5 kms and a dummy capturing whether the neighbors can be trusted. All control variables where measured at baseline. The matrix X_i is only included when specifically mentioned. The DC indicators on which we stratified the random allocation of the treatments enter our estimation via $DC_{i,s}$, where s is the DC.

We restrict our estimations to the ITT effect which gives us the average impact of offering the intervention on the PO level, regardless of whether the individual ultimately participates in joint marketing or not. The ITT effect is the relevant effect here since it is offering the treatment that affects the labor input decisions which are taken way before the final decision to join in the bulking of the PO. In other words, the sequencing is such that the labor decisions are affected by offering the intervention even if the member does not take them up in the end.

In addition, we estimate regressions where we control also for the initial value of the outcome variable. As proposed by (McKenzie, 2012) this specification increases statistical power when autocorrelation of the outcome variable is low:

$$Y_{i1} = \alpha + \beta CoD_i + \gamma IoS_i + \delta Both_i + \chi Y_{i0} + \sum_{s=1}^{10} \lambda_s DC_{i,s} + \epsilon_i \tag{3.2}$$

where Y_{it} captures the outcome at $t = 0, 1$, i.e. baseline or endline. The period of observation covers two agricultural seasons. We are explicitly interested in the distinct effects of the interventions on labor input for these two seasons. Therefore, we estimate all regressions separately for the first season - where labor decisions are taken without any experience about how the intervention changes access to markets - and the second season where members have some experience with the intervention.

We estimate all regressions with heteroskedasticity-robust standard errors. Secondly, we re-run the regressions and correct the standard errors through clustering on the PO level. The latter strategy takes into account that the regressors of interest, i.e. the treatment indicators, do not vary on the individual but only on the PO level. We cannot rule out that intraclass correlation is potentially high within the PO and when using robust standard errors its presence might lead us to overestimate the precision of our coefficients (Angrist and Pischke, 2008). The estimation results based on clustered standard errors are presented in Appendix A. Comparison with the robust estimations shows that the standard errors increase. Yet the significance of most results is robust to clustering.

3.4.3 Average Impacts on Household Labor Inputs

Table 3.4 presents coefficients from estimation of Equation 3.1 in Columns (1) and (2) and of Equation 3.2 in Column (3). The outcome variable is the number of household member labor days in farming activities. The left panel presents the coefficients estimated for that particular labor input in the first seasons, the right panel presents them for the second season. In the first season, the control group

reports having spent around 195 days in farming (see Table 3.2). Here, only assignment to the IoS treatment shows a significant effect, reflecting that the members in IoS POs increased the number of labor days their household spends farming by at least 54.4 days. Not only is this statistically significant, also from an economic point of view does the size play a significant role as on average every household member spends over one week more ($54.4/7 = 7.8$) in farming activities. When controlling for individual characteristics the effect is significant at the 5 per cent level. In the second season the estimated impacts of IoS are lower in size, ranging only from 43.4 to 55.7, and are no longer significant across all three specifications. The reduction in effect size could be explained by an unsatisfied experience made with the intervention during the first season. But this cannot be tested here. CoD has very low and insignificant effects ranging from 1 to 15.1 days in the first season. In the second season these effects are larger but remain insignificant.

Table 3.5 then looks at household labor days spent in crop production. Columns 1 through 3 of the middle panel show again that it is only assignment to the IoS group that has significant and sizable effects on the days the household spends on this activity in the first season. Again, these effects do not carry over to the second agricultural season. Yet, for household member days in crop production we observe large and significant effects from assignment to *Both*, i.e. the mix of both interventions. In this treatment group members increase input to this activity at least 48.6 days. These effects are significant at the 5 per cent level when estimated with individual level covariates and when we control for the initial value of the dependent variable. One could argue that the high increase in effect size from the first to the second season is a result from positive experience made during the first season. As for the earlier case, this cannot be tested here.

Table 3.4: OLS - Impact on Household Member Labor Days in Farming Activities

	First Season			Second Season		
	(1)	(2)	(3)	(1)	(2)	(3)
CoD	-3.2	13.6	-5.0	28.1	47.0	35.5
	(29.8)	(28.7)	(29.8)	(33.5)	(33.8)	(33.3)
IoS	47.1	63.8**	47.2	44.7	61.7*	46.4
	(32.6)	(31.8)	(32.3)	(33.9)	(34.0)	(33.4)
both	5.0	27.4	5.0	62.4	86.3**	67.0*
	(29.7)	(30.0)	(29.2)	(40.1)	(41.8)	(38.8)
Observations	317	311	317	316	310	316
R^2	0.061	0.148	0.072	0.112	0.194	0.157
Adjusted R^2	0.024	0.074	0.032	0.077	0.123	0.121
Individual level covariates	no	yes	no	no	yes	no
Initial value of Y as covariate	no	no	yes	no	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Robust standard errors in parentheses.

Table 3.5: OLS - Impact on Household Member Labor Days in Crop Production

	First Season			Second Season		
	(1)	(2)	(3)	(1)	(2)	(3)
CoD	2.9	5.1	3.3	16.7	21.5	16.0
	(14.4)	(14.9)	(14.4)	(18.3)	(19.3)	(18.5)
IoS	25.2*	27.2*	26.1*	33.1	35.0	36.1
	(14.7)	(14.9)	(14.8)	(21.7)	(23.1)	(21.9)
both	8.4	16.2	7.3	52.1**	59.5**	53.7**
	(13.4)	(14.3)	(13.4)	(23.9)	(25.7)	(23.8)
Observations	317	311	315	316	310	315
R^2	0.113	0.198	0.118	0.145	0.213	0.156
Adjusted R^2	0.078	0.128	0.080	0.111	0.144	0.119
Individual level covariates	no	yes	no	no	yes	no
Initial value of Y as covariate	no	no	yes	no	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Robust standard errors in parentheses.

3.4.4 Average Impacts on Hired Labor Inputs

A key question is not only whether households adapt their own labor input in anticipation of better market access but whether their demand for external labor is also affected. Table 3.6 examines the impact of assignment to one of the treatment groups on labor hired for farming activities. We see that there is no significant impact. The lack of an effect here might, in part, be explained by the fact that all four groups increase hired labor substantially from baseline to endline. The mean for the control group is at around 50 days in both of the two seasons which is about 20 days higher than at baseline (see Tables 3.2 and 3.3 in Section 3.4.1). For the other groups we even observe increases by 30 days. Given that we observe already significant increases in household labor for this activity, at least for the IoS group, one could also assume that this crowds out the need for hired labor.

In Table 3.7 we see the estimates for the impact on hired labor for crop production. Here the mean of the control group in the first season is 42 days (Table 3.2) and 58 days for the second season (Table 3.3). The OLS estimates of Equation 3.1 are all insignificant. For the first season the estimates of Equation 3.2, controlling for the initial value of the outcome, result in an impact of 36.8 days with significance at the 10 per cent level for the CoD and 50.5 days for the IoS treatment at 5 per cent significance. However, these estimates need to be considered apart as the number of observations is relatively low. This results from the fact that only 124 individuals hire laborers in both, the first and in the second season.

Table 3.6: OLS - Impact on Hired Labor Fays in Farming Activities

	First Season			Second Season		
	(1)	(2)	(3)	(1)	(2)	(3)
CoD	18.8	22.2	16.3	16.0	17.2	13.8
	(19.9)	(21.0)	(19.7)	(22.5)	(23.7)	(22.4)
IoS	20.7	20.8	11.8	6.1	4.8	-1.0
	(16.3)	(15.3)	(15.0)	(16.6)	(16.5)	(16.0)
both	-8.9	3.4	-7.1	12.2	26.0	12.5
	(15.4)	(17.4)	(14.4)	(27.5)	(29.7)	(27.2)
Observations	317	311	316	317	311	316
R^2	0.037	0.092	0.123	0.052	0.091	0.084
Adjusted R^2	-0.001	0.013	0.085	0.015	0.011	0.045
Individual level covariates	no	yes	no	no	yes	no
Initial value of Y as covariate	no	no	yes	no	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Robust standard errors in parentheses.

Table 3.7: OLS - Impact on Hired Labor Days in Crop Production

	First Season			Second Season		
	(1)	(2)	(3)	(1)	(2)	(3)
CoD	24.2	25.1	36.8*	35.6	31.3	53.7
	(17.5)	(18.9)	(21.5)	(29.4)	(32.2)	(41.4)
IoS	12.3	15.2	50.5**	8.9	10.6	34.9
	(14.7)	(13.5)	(19.7)	(20.7)	(20.7)	(24.1)
both	15.7	32.4	42.0	44.3	60.1	28.0
	(21.5)	(24.4)	(26.9)	(47.1)	(47.3)	(27.1)
Observations	185	183	124	181	179	117
R^2	0.040	0.123	0.177	0.106	0.133	0.134
Adjusted R^2	-0.027	-0.016	0.080	0.042	-0.009	0.024
Individual level covariates	no	yes	no	no	yes	no
Initial value of Y as covariate	no	no	yes	no	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Robust standard errors in parentheses.

3.4.5 Heterogeneity of Impacts

Naturally, trust plays an important role in the member's decision to adjust labor input as a result of the anticipated changes from the interventions. This is especially relevant, in the first season there is no experience on how these changes will be implemented. We therefore examine whether the effects of assignment to treatment differ for those members that have trust in their PO leaders at baseline. A question asking directly whether the respondent trusts the PO leaders risks to suffer from courtesy bias, i.e. the respondent gives socially desirable answers. Instead, we elicit the respondent's opinion of the PO leaders indirectly. At baseline, the respondents rated the effort that, in their opinion, the PO leaders would exert for a communal project [5]. This rating reflects to what degree the respondent esteems the PO leaders. We assume that this correlates with the level of trust in the PO leaders. We normalized this rating by the effort that the respondent expects from ordinary members and created a dummy variable taking on value one whenever the respondent thinks the PO leaders would put more effort into the project than the ordinary members. The following regressions include only the subsample of PO members who trust their PO leaders according to this variable.

Table 3.8 presents the effect on household member days in farming activities. As compared to the full sample the subgroup analysis shows similar results for the first season: Only assignment to IoS causes significant increases in the number of days the households spends on farming. The size of the effects is at around 70 days which is roughly 15 days higher than for the full sample. Interestingly, for the trustful sample we also detect significant effects of similar size for IoS assignment in the sec-

[5] At endline we ask directly whether the PO leaders can be trusted despite the risk of courtesy bias.

ond season. For the members assigned to receive both interventions simultaneously we obtain a negative but insignificant effect in the first season. CoD members only increase their household farming days insignificantly in the first season. For the second season, we estimate a significant impact of 62.2 days if we control for individual characteristics.

In Table 3.9 we find a similar pattern for crop production. The impact of IoS assignment that we observe to be significant in the full sample is stronger here in terms of magnitude and significance and it also remains significant in the second season. While the IoS treatment is propelled by trust in the PO leaders, assignment to CoD and *Both* does not lead to heterogeneous effects. On the contrary, assignment to *Both* has significant effects on the full sample but not on the subsample. The estimates of the impact on hiring labor for farming are all larger in absolute size. Striking is the large negative impact that assignment to *Both* causes in the first season. It is counter intuitive that the implementation of both interventions leads members to reduce their demand for hired labor, and even more so for those who have trust in their leaders. The significance of the effect is not robust across seasons or specifications, yet the sign remains negative.

The regression results presented in Table 3.11 capture the ITT effect on labor hired for crop production for the subsample of members that trust their PO leaders. We see that in the first season assignment to CoD increases hired labor input by 34.3 days and more, depending on the specification. These effects are substantially higher as compared to the estimates on the entire sample and they are also significant at the 5 per cent level. This indicates that the outlook to be eligible to receive cash on delivery has had stronger effects on those members that state at baseline to trust

their PO leaders. For the second season, we document even higher effects of over 40.5 days but these impact estimates are insignificant.

Table 3.8: Impact on Household Member Labor Days in Farming Activities (Sample of Respondents that Trust their PO Leaders)

	First Season			Second Season		
	(1)	(2)	(3)	(1)	(2)	(3)
CoD	7.8	24.9	5.9	41.4	59.6	48.6
	(34.1)	(31.6)	(34.0)	(38.6)	(38.1)	(38.0)
IoS	63.8*	78.2**	64.2*	59.3	75.8**	61.8*
	(36.0)	(34.9)	(35.7)	(37.4)	(37.2)	(36.6)
both	-19.9	4.7	-18.9	35.3	64.8	44.1
	(32.0)	(30.6)	(31.7)	(45.9)	(48.7)	(45.4)
Observations	265	260	265	264	259	264
R^2	0.076	0.179	0.085	0.085	0.181	0.128
Adjusted R^2	0.033	0.091	0.038	0.041	0.093	0.083
Individual level covariates	no	yes	no	no	yes	no
Initial value of Y as covariate	no	no	yes	no	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Robust standard errors in parentheses.

3.4.6 Average Impacts on Trust in Leaders

The previous section provided evidence that heterogeneity in the impact exist according to whether the member has trust in PO leaders or not. It is trust at baseline that creates this heterogeneity. Since we measure trust at endline as well, we can test whether any of the interventions affected it.

Table 3.12 documents the descriptive results for three different trust measures observed at endline. The respondents rated whether most people can be trusted, whether in their absence neighbors could be trusted to look after their house and whether PO leaders could be trusted to make decisions that are good for the PO members. All three original variables are scaled from 1 (strongly disagree) to 7 (strongly agree). Based on this we created indicator variables taking on value one

Table 3.9: Impact on Household Member Labor Days in Crop Production (Sample of Household that Trust their PO Leaders)

	First Season			Second Season		
	(1)	(2)	(3)	(1)	(2)	(3)
CoD	9.8	10.7	10.5	28.2	31.4	27.2
	(17.0)	(17.3)	(17.0)	(21.1)	(21.9)	(21.2)
IoS	36.0**	36.2**	37.5**	47.7**	47.6*	49.8**
	(16.6)	(16.8)	(16.8)	(24.1)	(25.4)	(24.2)
both	-0.8	6.9	-2.8	40.6	46.5	42.6
	(14.4)	(15.1)	(14.4)	(26.5)	(28.9)	(26.6)
Observations	265	260	264	264	259	264
R^2	0.105	0.202	0.113	0.106	0.191	0.113
Adjusted R^2	0.062	0.117	0.067	0.063	0.104	0.067
Individual level covariates	no	yes	no	no	yes	no
Initial value of Y as covariate	no	no	yes	no	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Robust standard errors in parentheses.

Table 3.10: Impact on Hired Labor Days in Farming Activities (Sample of Respondents that Trust their PO Leaders)

	First Season			Second Season		
	(1)	(2)	(3)	(1)	(2)	(3)
CoD	34.5	35.4	32.0	36.3	35.7	33.4
	(21.9)	(22.4)	(21.5)	(24.2)	(24.3)	(23.9)
IoS	35.9**	31.7*	25.3	24.0	18.2	15.7
	(17.6)	(16.8)	(15.8)	(15.5)	(15.2)	(14.7)
both	-19.0*	-9.0	-13.9	-11.2	-0.3	-8.6
	(11.2)	(11.7)	(11.4)	(12.3)	(13.9)	(12.0)
Observations	266	261	265	266	261	265
R^2	0.066	0.142	0.109	0.062	0.135	0.097
Adjusted R^2	0.021	0.051	0.063	0.018	0.043	0.050
Individual level covariates	no	yes	no	no	yes	no
Initial value of Y as covariate	no	no	yes	no	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Robust standard errors in parentheses.

Table 3.11: Impact on Labor Days of Hired Labor for Crop Production (Sample of Respondents that Trust their PO Leaders)

	First Season			Second Season		
	(1)	(2)	(3)	(1)	(2)	(3)
CoD	36.4**	38.8**	44.5*	52.3	51.7	72.2
	(18.4)	(19.5)	(23.8)	(31.6)	(34.7)	(48.0)
IoS	26.2*	25.4*	59.8***	21.8	20.9	51.9*
	(15.6)	(13.4)	(22.4)	(19.1)	(17.6)	(27.5)
both	-9.3	0.6	7.7	3.3	18.2	22.9
	(10.1)	(11.7)	(13.2)	(18.8)	(22.6)	(28.1)
Observations	156	154	105	152	151	100
R^2	0.079	0.237	0.130	0.115	0.194	0.139
Adjusted R^2	0.002	0.089	0.005	0.038	0.033	0.009
Individual level covariates	no	yes	no	no	yes	no
Initial value of Y as covariate	no	no	yes	no	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Robust standard errors in parentheses.

whenever an answer greater than 4 (neither agree nor disagree) was given. We present the mean level of trust for the respective group in the middle panel and p-values from t-tests comparing the means for the different treatment groups to the control group in the last panel. None of the trust indicators shows a significant difference. This indicates that trust as we measure it was not affected by any of the interventions.

Trust in PO leaders is very high at over 80 percent in all groups. While this variable was proxied at baseline (see Section 3.3.1), at endline we asked directly whether the PO leaders can be trusted. This could in part explain the higher average outcome at endline as compared to the baseline (compare Table 3.1). If one assumes that trust is constant over time, the difference between the proxy measure at baseline and the direct measure at endline could be interpreted as a naive estimate of the courtesy bias, i.e. the interview situation causes the respondent to rate the trust in PO leaders higher than she would rate it under other circumstances.

Table 3.12: Summary Statistics of Trust Measures at Endline

	Number of Observations				Mean of Variable				p-value of t-test with control group		
Variable Names	CoD	IoS	Both	Control	CoD	IoS	Both	Control	CoD	IoS	Both
Trust most people	82	85	77	75	.62	.46	.55	.57	.54	.15	.73
Trust neighbours	82	85	77	75	.76	.71	.75	.81	.39	.12	.37
Trust PO leaders	82	85	77	75	.85	.86	.81	.83	.65	.58	.73

Note: Variables are indicators taking value one if respondent agrees to statement.

We assess the impact on trust equally by estimating equation 3.1 with and without individual level covariates. The question on trust in PO leaders differs between baseline and endline so that we are not able to estimate Equation 3.2. The coefficient estimates are presented in Table 3.13. We find no evidence for a positive effect on trust from any of the interventions. Our impact estimates are fairly small in size, amounting at most to 3 per cent, and are all statistically insignificant. This confirms the descriptive picture that trust in the PO leaders, as we measure it, was not affected by the intervention.

Table 3.13: OLS - Impact on Trust in PO Leaders

	(1)	(2)
CoD	0.03	0.03
	(0.1)	(0.1)
IoS	0.02	0.02
	(0.1)	(0.1)
both	-0.03	-0.02
	(0.1)	(0.1)
Observations	319	313
R^2	0.048	0.066
Adjusted R^2	0.011	-0.016
Individual level covariates	no	yes
Initial value of Y as covariate	no	no

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Robust standard errors in parentheses. Specification (3) not estimated as variable was not measured at baseline. All estimations control for stratification on the DC level.

3.5 Conclusion

Our results provide further evidence for the relevance of collective action for rural development. We have shown that interventions aiming at improved market access through producer organizations in rural Africa can substantially affect the labor input decisions of the PO members. The results support the assumption that lack of market accesss creates disincentives for labor input in smallholder agriculture,

which potentially hampers development. We observe that tackling the imperfections that block market access can unleash labor resources. Sadoulet et al. (1998) describe three different labor regimes in agricultural economies: wage laborers, farmers relying solely on household labor and farmers that hire in additional labor. Our results suggest that the interventions we study can make a difference for all three regimes. First, the provision of information to increase transparency in the sales procedures of the PO proved effective in unleashing untapped household labor. Second, we provide some evidende that the reduction of cash constraints at harvest time allows for increases in the employment of hired labor.

Especially so for the latter, we document that the effects are stronger when the level of trust in PO leaders is high. This demonstrates the importance of strong institutional features that enable the PO to address the members' needs. One can assume that members' trust correlates with institutional strength. We observe further evidence for effect heterogeneity as the effects wear off in the full sample after the first season. The impact persists over time only in the subsample of members who trust their PO leaders. This gives rise to assess whether the interventions were effective in changing the members' trust in their institution. We fail, though, to provide evidence on this end.

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Appendix

Table 3.14: OLS - Impact on Household Member Labor Days in Farming Activities

	First Season			Second Season		
	(1)	(2)	(3)	(1)	(2)	(3)
CoD	-3.2	13.6	-5.0	28.1	47.0	35.5
	(35.5)	(32.4)	(35.4)	(40.6)	(38.0)	(39.9)
IoS	47.1	63.8*	47.2	44.7	61.7	46.4
	(37.8)	(36.3)	(37.6)	(40.0)	(38.6)	(39.3)
both	5.0	27.4	5.0	62.4	86.3*	67.0
	(30.7)	(29.1)	(30.4)	(47.6)	(48.3)	(45.6)
Observations	317	311	317	316	310	316
R^2	0.061	0.148	0.072	0.112	0.194	0.157
Adjusted R^2	0.024	0.074	0.032	0.077	0.123	0.121
Individual level covariates	no	yes	no	no	yes	no
Initial value of Y as covariate	no	no	yes	no	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses clustered at PO-level.

All estimations control for stratification on the DC level.

Table 3.15: OLS - Impact on Household Member Labor Days in Crop Production

	First Season			Second Season		
	(1)	(2)	(3)	(1)	(2)	(3)
CoD	2.9	5.1	3.3	16.7	21.5	16.0
	(15.1)	(14.9)	(15.1)	(20.8)	(20.8)	(20.8)
IoS	25.2	27.2*	26.1	33.1	35.0	36.1
	(16.5)	(16.1)	(16.7)	(24.9)	(25.8)	(25.2)
both	8.4	16.2	7.3	52.1*	59.5**	53.7**
	(12.9)	(13.7)	(13.1)	(26.9)	(28.6)	(26.8)
Observations	317	311	315	316	310	315
R^2	0.113	0.198	0.118	0.145	0.213	0.156
Adjusted R^2	0.078	0.128	0.080	0.111	0.144	0.119
Individual level covariates	no	yes	no	no	yes	no
Initial value of Y as covariate	no	no	yes	no	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses clustered at PO-level.

All estimations control for stratification on the DC level.

Table 3.16: OLS - Impact on Hired labor Days in Farming Activities

	First Season			Second Season		
	(1)	(2)	(3)	(1)	(2)	(3)
CoD	18.8	22.2	16.3	16.0	17.2	13.8
	(22.8)	(23.8)	(22.4)	(25.8)	(26.2)	(25.7)
IoS	20.7	20.8	11.8	6.1	4.8	-1.0
	(18.7)	(18.0)	(17.6)	(19.3)	(19.0)	(18.9)
both	-8.9	3.4	-7.1	12.2	26.0	12.5
	(16.2)	(18.5)	(15.9)	(28.2)	(30.5)	(28.1)
Observations	317	311	316	317	311	316
R^2	0.037	0.092	0.123	0.052	0.091	0.084
Adjusted R^2	-0.001	0.013	0.085	0.015	0.011	0.045
Individual level covariates	no	yes	no	no	yes	no
Initial value of Y as covariate	no	no	yes	no	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses clustered at PO-level.

All estimations control for stratification on the DC level.

Table 3.17: OLS - Impact on Hired Labor Days in Crop Production

	First Season			Second Season		
	(1)	(2)	(3)	(1)	(2)	(3)
CoD	24.2	25.1	36.8	35.6	31.3	53.7
	(19.0)	(19.8)	(23.3)	(28.8)	(30.7)	(40.8)
IoS	12.3	15.2	50.5**	8.9	10.6	34.9
	(16.4)	(14.7)	(24.1)	(24.0)	(23.7)	(27.8)
both	15.7	32.4	42.0	44.3	60.1	28.0
	(20.1)	(22.5)	(27.2)	(47.2)	(47.2)	(28.0)
Observations	185	183	124	181	179	117
R^2	0.040	0.123	0.177	0.106	0.133	0.134
Adjusted R^2	-0.027	-0.016	0.080	0.042	-0.009	0.024
Individual level covariates	no	yes	no	no	yes	no
Initial value of Y as covariate	no	no	yes	no	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses clustered at PO-level.

All estimations control for stratification on the DC level.

Table 3.18: Impact on Household Member Labor Days in Farming Activities (Sample of Respondents that Trust their PO Leaders)

	First Season			Second Season		
	(1)	(2)	(3)	(1)	(2)	(3)
CoD	7.8	24.9	5.9	41.4	59.6	48.6
	(40.7)	(36.2)	(40.6)	(46.8)	(42.8)	(45.2)
IoS	63.8	78.2*	64.2	59.3	75.8*	61.8
	(42.3)	(40.0)	(42.0)	(43.9)	(41.3)	(42.8)
both	-19.9	4.7	-18.9	35.3	64.8	44.1
	(35.9)	(32.3)	(35.6)	(51.2)	(51.7)	(50.4)
Observations	265	260	265	264	259	264
R^2	0.076	0.179	0.085	0.085	0.181	0.128
Adjusted R^2	0.033	0.091	0.038	0.041	0.093	0.083
Individual level covariates	no	yes	no	no	yes	no
Initial value of Y as covariate	no	no	yes	no	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses clustered at PO-level.

All estimations control for stratification on the DC level.

Table 3.19: Impact on Household Member Labor Days in Crop Production (Sample of Respondents that Trust their PO Leaders)

	First Season			Second Season		
	(1)	(2)	(3)	(1)	(2)	(3)
CoD	9.8	10.7	10.5	28.2	31.4	27.2
	(18.0)	(17.8)	(18.1)	(23.8)	(23.7)	(23.7)
IoS	36.0*	36.2**	37.5**	47.7*	47.6*	49.8*
	(18.3)	(17.6)	(18.7)	(27.2)	(27.7)	(27.2)
both	-0.8	6.9	-2.8	40.6	46.5	42.6
	(14.9)	(14.9)	(15.0)	(28.9)	(30.4)	(29.1)
Observations	265	260	264	264	259	264
R^2	0.105	0.202	0.113	0.106	0.191	0.113
Adjusted R^2	0.062	0.117	0.067	0.063	0.104	0.067
Individual level covariates	no	yes	no	no	yes	no
Initial value of Y as covariate	no	no	yes	no	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses clustered at PO-level.

All estimations control for stratification on the DC level.

Table 3.20: Impact on Hired Labor Days in Farming Activities (Sample of Respondents that Trust their PO Leaders)

	First Season			Second Season		
	(1)	(2)	(3)	(1)	(2)	(3)
CoD	34.5	35.4	32.0	36.3	35.7	33.4
	(24.9)	(25.1)	(24.4)	(28.2)	(27.9)	(28.1)
IoS	35.9*	31.7	25.3	24.0	18.2	15.7
	(20.6)	(19.3)	(18.7)	(18.8)	(17.6)	(18.0)
both	-19.0	-9.0	-13.9	-11.2	-0.3	-8.6
	(12.7)	(13.1)	(13.1)	(14.0)	(15.3)	(13.8)
Observations	266	261	265	266	261	265
R^2	0.066	0.142	0.109	0.062	0.135	0.097
Adjusted R^2	0.021	0.051	0.063	0.018	0.043	0.050
Individual level covariates	no	yes	no	no	yes	no
Initial value of Y as covariate	no	no	yes	no	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses clustered at PO-level.

All estimations control for stratification on the DC level.

Table 3.21: Impact on Labor Days of Hired Labor for Crop Production (Sample of Respondents that Trust their PO Leaders)

	First Season			Second Season		
	(1)	(2)	(3)	(1)	(2)	(3)
CoD	36.4*	38.8*	44.5*	52.3*	51.7	72.2
	(20.1)	(20.9)	(24.7)	(31.0)	(35.2)	(45.4)
IoS	26.2	25.4	59.8**	21.8	20.9	51.9*
	(17.8)	(15.5)	(26.3)	(22.4)	(20.7)	(31.0)
both	-9.3	0.6	7.7	3.3	18.2	22.9
	(12.2)	(12.9)	(15.1)	(20.6)	(24.3)	(30.7)
Observations	156	154	105	152	151	100
R^2	0.079	0.237	0.130	0.115	0.194	0.139
Adjusted R^2	0.002	0.089	0.005	0.038	0.033	0.009
Individual level covariates	no	yes	no	no	yes	no
Initial value of Y as covariate	no	no	yes	no	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses clustered at PO-level.

All estimations control for stratification on the DC level.

CHAPTER IV

The Impact of a Formalization Intervention in India

4.1 Introduction

Informality of large parts of the workforce and the majority of self-employed entrepreneurs is a characteristic of most developing country economies. In India, informal self-employment accounts for 92 percent of all self-employment in rural and for 95 percent in urban areas (NSSO, 2012). Claims exist that these unregulated segments of the economy create disadvantages on several grounds (Gërxhani, 2004). Regarding economic development, the large share of entrepreneurial activities outside of the tax system curtails the public sector's capacity to provide goods and services. As for social security, a large informal sector is directly opposed to the inclusion of workers to the benefit schemes.

Therefore, developing country governments, development academics and practitioners have strong interest in the inclusion of unregistered, informal activities. Yet, most research on the informal sector has focused on explaining its existence (de Soto, 1989; Gelb et al., 2009; Loayza et al., 2005). The work of Perry et al. (2007) identifies two rationales underlying informality. Under the first, entrepreneurs *exit* formality consciously, i.e. they operate informally after weighing the pros and cons of formal-

ization. The second rationale, called the *exclusion view*, presumes that high direct and indirect costs and burdensome processes render the formal sector inaccessible for microentrepreneurs, forcing them to remain "in the shadows". Though not mentioned in the literature, the two processes can work simultaneously, too.

In large parts, the existing literature on formalization studies how changes in the regulatory framework or increased enforcement thereof affect the size of the informal sector (Almeida and Carneiro, 2012; Bruhn, 2011; Fajnzylber et al., 2011; Ihrig and Moe, 2004; Loayza, 1994). This approach rather addresses informality as a result of entrepreneurs exiting formality. With the rise of rigorous impact evaluation in development economics, researchers started to assess how benevolent manipulation affects the clients' formalization decision. de Mel et al. (2013) test the exclusion theory in Sri Lanka and document the irrelevance of formalization for microentrepreneurs given its costs and the prospects it offers. They find no effect on formalization when all costs for licensing fees are reimbursed and basic information is provided. It is only when entrepreneurs are paid additional money for registering that formalization increases significantly. This suggests either that mere registration is not sufficiently interesting or that the indirect costs attached to registering are non-negligible.

The present work looks at the effect of reducing the indirect costs of formalization. At the focus of the study is the Evangelical Social Action Forum (ESAF), an Indian NGO that pilot tested an intervention to foster formalization among its microfinance clients, henceforth called 'the clients'. The intervention explains in detail the advantages of formalization for the self-employed. In addition, the beneficiaries were actively encouraged and supported in their efforts to register.

In summary, our findings document that once information on benefits and procedures are provided and some support to register is given, formalization rates soar among the group of clients that ESAF identified as "ready-to-grow" clients. Using a difference-in-difference estimation strategy we estimate substantial and significant effects on registration. The large share of treated clients that decided to formalize allows us to test whether subsequent outcomes were affected by formalization. Here the results are disappointing as we find very few significant effects. We do not find any impact on annual turnover, on investment in new machinery, on electrification of the business, on ownership of bank accounts or insurance of the business, nor on the household's monthly income.

We find positive and significant effects on a series of outcome indicators that are related to the perception of the business. We find a significant positive impact on plans to expand the business. We find significant positive impact on the perceived role that the enterprise plays for the household. And we find a positive effect on advertisement activities.

When it comes to employment we observe that the number of clients who hire employees decreases significantly. This suggests either that entrepreneurs are not reporting their employees, as they might not have reported them to the authorities, or that registration truly gives incentives to reduce the number of employees in the microenterprise.

The paper proceeds by giving background information on the context, the implementing agency and the intervention in section 2. In section 3 we discuss the impact evaluation design, the data collection and the sample. Section 4 presents the results and the last section concludes.

4.2 Context and the Intervention

The informal sector comprises all income generating activities that are not registered with official authorities. The official authorities are industries centers, tax agencies and the local government. In 2008, ESAF conducted a study among 200 of its clients to gather information on work related challenges that they encounter. This study revealed that 85 percent of the self-employed clients did not have any kind of registration of their activity; 69 percent of the self-employed did not conduct even basic bookkeeping such as maintaining notes of income and expenditure; 88 percent of the self-employed clients did not pay taxes. Of the clients who had employees only 24 percent provided their staff with some kind of social benefits, usually paid on the basis of ad-hoc and informal arrangements (ILO, 2008).

And most importantly, the clients were not aware of the benefits available for formal entrepreneurs. The Indian governments has put in place incentive schemes to render formalization beneficial for small and micro-entrepreneurs: Tax reduction, reduction of the costs for licenses for poor entrepreneurs, subsidies for training or business related expenses.

In developing countries this is a common picture. Yet, ESAF assumes that a number of its informal self-employed clients has the potential to expand their enterprise, but due to their informal status they could not access bank services or government support schemes. As a consequence ESAF developed a strategy to support ready to grow clients in the formalization process.

4.2.1 Formalization and its Benefits

ESAF considers formalization as an important factor for the growth of enterprises. Formality allows access to loans from banks that are larger than the micro credits

that can be obtained from ESAF. Further, formalization allows access to subventions through government schemes. The requirement to maintain and submit business information regularly clearly is a burden but forces formal enterprises to closely monitor their activities which can have positive effects on business management. The improved business practices along with a better access to finance and markets opens up business expansion avenues for formal enterprises that remain closed under informal activity.

On the other hand, ESAF is aware of the financial burden that formalization implies. Formality requires entrepreneurs to pay relevant taxes and to submit financial documents. The taxes combined with the costs for registering add up to the enterprise's non-operational expenses and discourage entrepreneurs to formalize.

4.2.2 The Intervention

The implementing agency, ESAF, reckons that for a certain group of its clients the main obstacles to formalize are low awareness about the advantages, overrating of the disadvantages and a lack of information on the formalization process rather than a lack of funds to cover the costs induced by formalization. The latter includes the upgrades of business processes that are necessary to be eligible for registration. To address issues of formality and support the business expansion of its ready-to-grow clients, ESAF implemented awareness raising campaigns to explain registration and formalization processes and created business development services (BDS) for its clients. The issue of the costs of formalization was left to the microentrepreneur.

To provide the specific non-financial services, ESAF created a new capacity among its staff: the business development officer (BDO). BDOs are responsible for collect-

ing and elaborating information materials on registration, facilitating sensitization workshops at branch level and providing individual counseling to the clients.

ESAF recruited three BDOs and appointed one to each of the three states that were included in the impact evaluation: Kerala, Maharashtra and Tamil Nadu. The BDOs conducted 10 information campaigns to promote formalization and explain the registration process to the clients. The campaigns were conducted from May until August 2010 and reached 410 selected clients. Besides posters and meetings the campaigns included the following programs:

In the treatment branches, training sessions were held with batches of 30-40 clients. These served the purposes of introducing to and sensitizing the clients on formalization. During these sessions it was explained in detail how to formalize a microenterprise through registration with the local district industries center (DIC) and government agencies. The clients were also encouraged to expand their markets through fairs or exhibitions.

At individual client visits the BDO reiterated the messages on the benefits of formalization. And during regular interaction at weekly meetings the clients received briefing sessions on marketing and accounting.

4.3 Evaluation Design, Data Collection and Estimation Strategy

In April and May 2010 a baseline survey was conducted in twelve branches with a total of 670 respondents. From the six treatment branches 340 clients were interviewed and from the six control branches 330 clients were interviewed. Overall, the majority of respondents were female. In Tamil Nadu and Kerala they were exclu-

sively females, while in Maharashtra the majority was male. All respondents were loan clients.

In intervals of six months three follow-up surveys were conducted allowing us now to construct a panel data set. Attrition, i.e. drop-out of respondents, is a relevant problem especially in the treatment group where we observe around 10 percent of attrition between baseline and the first follow-up survey, henceforth follow-up I. In the control branches on the other hand the number of respondents even increased by 10 percent. At the second follow-up survey we observe an increase in the treatment sample to the level of baseline. The control group sample at follow-up II shows an attrition of around 6 percent compared to the baseline. For the analysis we use only individuals who were present at all three interviews.

The initial evaluation design called for a cluster randomized trial where within each of the three regions two branches would have been allocated at random to each treatment and control. In other words, stratification by region was planned initially. This would have assured that observable and unobservable characteristics within the three regions were similar in the two groups. Of course treatment allocation on the branch level is to be applied in this context as random allocation on the individual level would result in spill-over effects within branches. This kind of contamination of the control group would have posed a threat to the internal validity of the study.

Within the respective branches only clients with very specific characteristics qualified for formalization and subsequently for the treatment as well. This revealed to influence also the allocation of the branches. Instead of four branches there were only three branches with a sufficient number of eligible clients in the region Tamil Nadu. In Kerala, seven instead of four branches had to be included in order to reach

the number of eligible clients. In Maharashtra on the other hand only two branches were included, both with a sufficiently high number of eligible clients.

4.3.1 Identification Strategy

The availability of data collected before and data collected after implementation of the intervention allows us to apply a double difference identification strategy[1]. This method, also called difference-in-difference estimation, differs from single difference methods that estimate the impact of an intervention by comparing outcomes of treated and untreated individuals only after implementation.

Single difference methods rely on the assumption that it is possible to rule out unobservable differences between the treated and untreated by controlling for observable characteristics, i.e. conditional on covariates X one assumes exogeneity of the treatment[2]: $Y^d \perp D|X$. Where Y^d represents the individual's potential outcome under treatment status $D \in \{0, 1\}$. The actual outcome Y_t can be observed at $t = 0, 1$. We generally omit index i for the individual.

The identification strategy of the double difference method is to cancel out differences existing at baseline under the assumption that these are constant over time. In other words, the differences that exist at baseline would persist over time and both groups would experience "parallel" development if the intervention does not take place. In addition to the assumption of parallel time trends, we need to assume that measurement error, if present, affects both groups in the same way.

[1] This subsection draws on Ravallion (2008).
[2] The notation draws on Frölich (2008).

We obtain the double difference estimate (DD), by subtracting the difference between control group outcomes at follow-up and baseline from that of the treatment group:

$$DD = E(Y_1^1 - Y_0^1|D_1 = 1) - E(Y_1^0 - Y_0^0|D_1 = 0) = E(G_1|D_1 = 1) \tag{4.1}$$

Obviously, $Y_{i0}^1 = Y_{i0}^0 = Y_{i0}$ since the intervention has not yet taken place at $t = 0$.

Under the assumption that

$$E(Y_1^0 - Y_0^0|D_1 = 1) = E(Y_1^0 - Y_0^0|D_1 = 0) \tag{4.2}$$

we obtain the average treatment effect on the treated $E(G_1|D_1 = 1)$ as an estimate of the intervention's true impact $G_{i1} = Y_{i1}^1 - Y_{i1}^0$ which relies on the counter factual that cannot be observed for one individual i.

Ravallion (2008) explicitly points out the scenario where no change over time occurs in the control group, $E(Y_1 - Y_0|D_1 = 0) = 0$. In this special case the DD-estimate is equal to a before-and-after comparison of control group outcomes.

Given that imbalances exist between the two groups in our sample we employ the difference-in-difference strategy to evaluate the impact of the formalization treatment. This allows us to estimate causal effects even in the presence of pre-treatment imbalances between treatment and control group.

4.3.2 Baseline Information by Treatment Status

Table 4.1 shows summary statistics for the treatment and control group before the launch of the intervention. The sample consists mainly of married women from households of around 4.5 members. The clients' age across treatment and control group is fairly balanced. Just in the youngest age bracket the control branches have a significantly higher share with 15 percent of clients being of age 18 to 27 compared

to only 3 percent in the treatment group. It also shows that the treatment clients are older on average when we look at the share of clients in age group 48 to 57 where we observe a share of 20 percent at baseline compared to 11 percent in the control group. Imbalances between the two groups exist also in education. The control group has a higher share of clients with higher secondary education and a lower share of clients who have only primary education.

Table 4.1: Summary Statistics by Treatment Group

	Mean		Mean	t-test
Variable Names	Treated	Control	-difference	p-value
Client information				
Female	.85	.72	.13	.00
Married	.91	.85	.06	.03
Household(HH) size	4.47	4.36	.11	.41
HH members earning income	1.97	1.76	.2	.01
Age of client				
18 to 27	.05	.15	-.09	.00
28 to 37	.34	.39	-.05	.28
38 to 47	.4	.34	.06	.18
48 to 57	.2	.11	.09	.01
above 58	.01	.02	.00	.75
Client's education				
No formal schooling	.07	.07	.00	.98
Primary	.15	.07	.08	.01

Continued on next page...

... table 4.1 continued

Variable Names	Mean Treated	Mean Control	Mean -difference	t-test p-value
Upper Primary	.25	.19	.06	.12
Secondary	.39	.4	-.01	.83
Higher Secondary	.1	.16	-.06	.05
Client's type of activity				
Agriculture	.02	.04	-.01	.36
Hotel/Restaurant	.14	.11	.03	.25
Mobile trading	.07	.02	.05	.01
Production	.08	.13	-.05	.09
Services	.03	.11	-.08	.00
Trade/Commerce	.44	.44	.01	.84
Sample Size	245	257		

With regard to comparability we observe significant differences in age as well as in education across treatment and control group. However, for both variables these differences concern smaller parts of the sample. I.e. the most prevalent age groups, 28 to 37 years and 38 to 47 years, are balanced across treatment and control groups. Only for the age brackets with few observations we observe significant differences. For education it is similar. Upper primary and secondary education comprise of over 60 percent of the sample in both groups. And only the less frequent education levels show significant differences. Higher secondary education for example is five percent

higher in the control group at baseline. Interestingly, it does not remain constant in the control group and rather increases.

When it comes to the type of activity, a majority of over 44 percent of the entire sample engages in trade and commerce activities. The prevalence of agricultural activities is low with only two percent in both groups. We observe significant differences between the two groups for services and mobile trading[3]. Services are more frequent in the control group while mobile trading is in the treatment branches. Also the share of clients who created their business in partnership with somebody is fairly similar in both groups. The share of clients who had some training for their business is also equally high across the two groups. However, when it comes to the funds that were used to create the business we see that the share of entrepreneurs who used their own funds is about 30 higher in the treatment group.

Table 4.2 presents summary statistics for the clients' enterprise at baseline. Slightly above 30 percent of the respondents own a production unit[4]. The average age of the business is around seven to eight years. Most businesses were created by one person and only very few are endeavors that the client started in cooperation with a partner. The share of people who had any specific training before the creation of the business is below 20 percent at baseline.

[3]Services include mostly small street side hotels, food canteen, tailoring units and electrical and electronics repair and service centers.

[4]A production unit is a category of microenterprises which are involved in making food, detergents, soaps, phenol, ready made garments, jewelery, bags, brooms, handicrafts, manufacture of nuts, bolts and other similar activities.

Table 4.2: Summary Statistics of Client Enterprise by Treatment Group

	Mean		Mean	t-test
Variable Names	Treated	Control	-difference	p-value
Owns production unit	.4	.32	.08	.05
Age of business	8.54	8.16	.38	.27
Opened business in partnership	.16	.15	.01	.73
Opened business after special training	.16	.19	-.04	.29
Sample Size	245	257		

The imbalances that we observe call for a difference-in-differences estimation. We explain the estimation strategy in the following section.

4.3.3 Estimation Strategy

This section presents the empirical strategy applied for the analysis of the intervention's impact on a series of outcome indicators. The ideal impact assessment would measure the net difference in the outcome variables between randomly assigned treatment and control individuals. In the present case six branches were selected for implementation of the intervention and six branches were allocated to the control group. Within the treatment branches the intervention was available to all clients. Hence, the variation is on branch-level, not on the individual level. In absence of random assignment of the intervention to ESAF's clients a series of other evaluation methods are available to identify the effect on the treatment clients. E.g.

a naive estimate of the impact would simply compare the levels of outcome variables of interest before and after the intervention was conducted. Of course, this estimate would only require information on clients from treatment branches. The drawback of this strategy lies in the incapacity to tell whether the resulting difference is solely attributable to the intervention. Other (unobservable) factors could have affected the outcome variables and hence lead to an over- or underestimate of the intervention's impact.

The availability of baseline information, i.e. information before the intervention was implemented, and information gathered at several points in time after the intervention allows us to follow a difference-in-difference (DD) evaluation strategy. We obtain the *DD* estimator by ordinary least squares (OLS) regression on the pooled sample, i.e. estimating the following equations for all observations ($i = 1, ..., n$) from all points in time ($t = 0, 1, 2, 3$):

$$Y_{it} = \alpha + DD(D_i I(t > 0)) + \beta D_i + \sum_{p=1}^{3} \gamma_p t_p + \epsilon_{it} \tag{4.3}$$

The outcome variable is Y_{it}. The dummy variables t_p are period-fixed effects. For individual i we observe every variable at time $t = 0, 1, 2, 3$. In equation 4.3 the coefficient of interest is DD. It captures the effect of an interaction term between the dummy variable D_i, that is equal to one for individuals from treatment branches, and an indicator for observations from post-treatment periods, $I(t > 0)$. In our regression tables we label this coefficient *overall treatment effect* and display it in Column (1). Its estimation does not take into account individual client or business characteristics. In Column (2) of the estimation tables we present the overall treatment effect when such control variables are included in the estimation:

$$Y_{it} = \alpha + DD(D_i I(t > 0)) + \beta D_i + \sum_{p=1}^{2} \gamma_p t_p + \delta X_{it} + \epsilon_{it} \tag{4.4}$$

In Columns (3) and (4) of the estimation tables we present estimates of period-specific DD treatment effects, DD^q for $q = 1, 2$. This means we estimate coefficients that indicate the impact at follow-up I survey and follow-up survey II:

$$Y_{it} = \alpha + \sum_{q=1}^{2} DD^q (D_i t) + \beta D_i + \sum_{p=1}^{3} \gamma_p t_p + \epsilon_{it} \tag{4.5}$$

Again we provide estimates of these effects with and without individual control variables. Though not presented here, we further generalize the model in equation 4.5 to include also a term to estimate DD^3. This coefficient will be presented in Column (5) without covariates and Column (6) with covariates.

In all regression tables we present coefficient estimates and the respective p-values to document the level of significance. A p-value below 0.05 indicates that the distance between estimate and zero exceeds two standard deviations. All regressions apply heteroskedasticity robust standard errors. The control variables included are gender of the client, marital status, number of household members and number of household members who earn any income.

4.4 Results

This section presents the empirical analysis of ESAF's formalization intervention. It starts off with a presentation of summary statistics for the treatment and control group. Throughout this section the tables presenting descriptive results contain the number of observations in both groups, estimates of the mean for the variable under

consideration, the difference in means, i.e. mean of treatment group minus the mean of the control group, and p-values from a test for statistical significance in means.

4.4.1 Impact on Formalization

In this section we assess the impact on knowledge about formalization and on the status of registration. Entrepreneurs in Kerala and Tamil Nadu can register their activities with the directorate of industries and commerce (DIC). In Maharashtra, registration is either with the Department of Labor or with the Municipal Corporation. In addition to the registration status, we look at the take up of services and products that formalization supposedly eases, such as financial products or government support schemes.

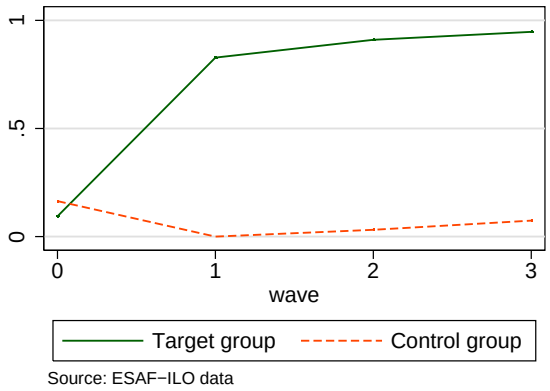

Figure 4.1: Do you know about Formalization?

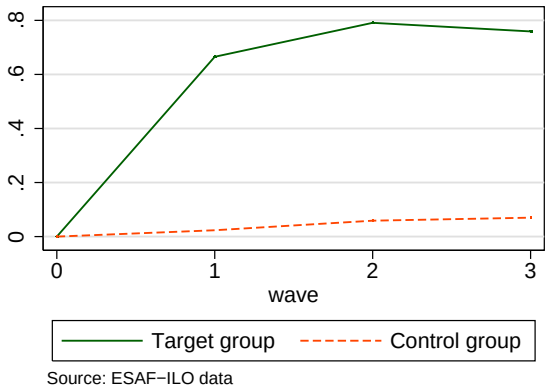

Figure 4.2: Have you registered your business?

Figure 4.1 provides a graph of the share of clients that state that they know about formalization for each survey round. It shows that awareness about formalization is low at baseline in both groups. At follow-up I ESAF's activities to inform about and promote formalization improved awareness as the share of informed clients increases to 80 percent while it remains low in the control group.

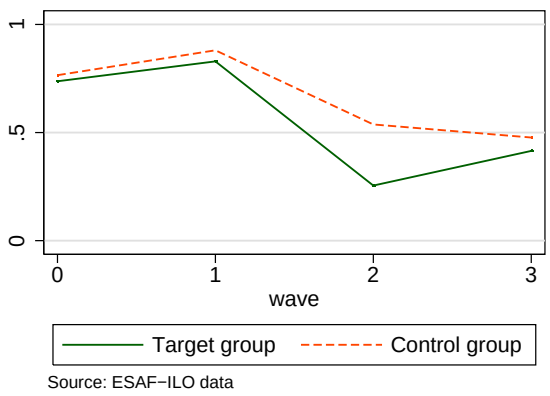

Figure 4.3: Do you wish to register your business?

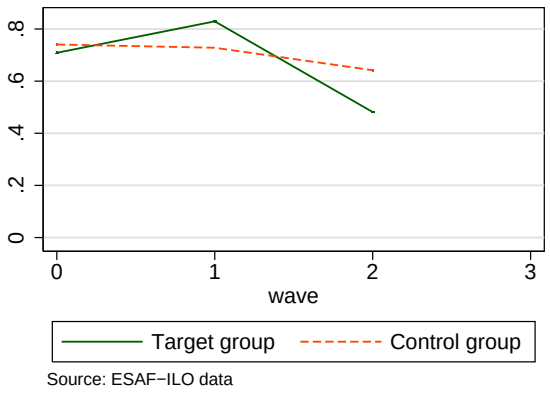

Figure 4.4: Are you willing to pay for the registration?

Figure 4.2 shows the share of clients that registered at the four different points in time that we observe. The sample consists strictly of informal clients at baseline, and we observe that the business registrations increase to over 60 percent among the treated clients at follow-up I. In the control group registrations remain rare. This is despite the fact that at baseline almost 80 percent of the control respondents stated that they would like to register (Figure 4.3). At baseline we also observe that those planning to register are willing to do so even if registration comes at a cost. The lack of registrations in the control group despite high initial willingness to register suggests that ESAF's assistance services contributed to the high rate of formalization in the treatment group.

Table 4.3: Impact on Formalization

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Knows about formalization						
Overall treatment effect	.93	.929				

Continued on next page...

... table 4.3 continued

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
	.00	.00				
Treatment effect FS I			.898	.897	.853	.897
			.00	.00	.00	.00
Treatment effect FS II			.949	.948	.904	.948
			.00	.00	.00	.00
Treatment effect FS III					.853	.943
					.00	.00
Registered business						
Overall treatment effect	.688	.687				
	.00	.00				
Treatment effect FS I			.642	.642	.677	.641
			.00	.00	.00	.00
Treatment effect FS II			.732	.732	.767	.731
			.00	.00	.00	.00
Treatment effect FS III					.759	.688
					.00	.00

Note: This table reports regression coefficients and p-values. The inference is based on heteroskedasticity-robust standard errors. The regressions reported in columns (1),(2),(5),(6) are based on all observations from all waves (pooled sample). The regressions reported in columns (3) and (4) exclude follow-up III observations. Individual covariates are included in regressions reported in columns (2),(4) and (6)

The empirical analysis confirms the descriptive picture. The formalization intervention had an important effect on awareness and registrations. We estimate an impact of 93 percent on awareness about issues of formalization and of 68.9 percent on actual registrations. As indicated by the p-values in the last column of Table 4.3 both effects are significant at the 1 percent level.

Access to Financial Products

Formalization can ease access to a range of services and products inaccessible when operating informally. At the outset slightly more respondents avail of a bank account in the treatment branches. Figure 4.5 presents the share of clients who hold a bank account. The trend over time is positive for both groups. After follow-up I the share of control clients with bank account still grows but less pronounced than in the treatment branches. Table 4.4 presents p-values of a difference-in-means tests in Column 6. It shows that there are significant differences in bank account ownership at follow-up I and follow-up II, so that the intervention has increased access to formal financial institutions.

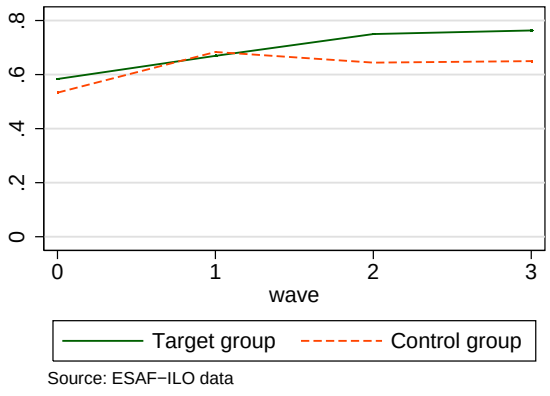

Figure 4.5: Do you have a bank account?

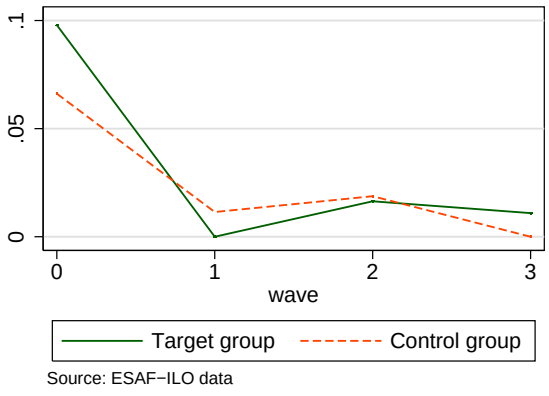

Figure 4.6: Business bank account

The clients were also asked whether they use their bank account for personal matters only, for business matters only, or for both. We observe that the accounts

are rarely used only for professional reasons. Figure 4.6 reveals that at baseline bank accounts opened under the name of the business activity are rare. In both groups roughly ten percent avail of accounts with a formal bank at baseline and this share drops to almost zero at later points. One possible explanation for the decrease in this share could be increased awareness about what a separate bank account is. The drop being more steep in the treatment branches might indicate that the formalization increased understanding in the issue. Since we rule out contamination of the control group an explanation for the decrease in the share of business bank accounts in the control group might be that interviewing itself raised awareness of the respondents causing them to revise their response during later interviews.

Table 4.4: Summary Statistics for Use of Financial Products by Treatment Group

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
Do you own a bank account? (0/1)						
Baseline	245	257	.58	.53	.05	.25
FS I	245	256	.67	.68	-.01	.73
FS II	244	253	.75	.64	.11	.01
FS III	245	257	.76	.65	.11	.01
Business bank account? (0/1)						
Baseline	143	136	.1	.07	.03	.34
FS I	163	175	.00	.01	-.01	.17
FS II	183	160	.02	.02	.00	.87

Continued on next page...

... table 4.4 continued

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
FS III	183	167	.01	.00	.01	.18
Is your firm insured? (0/1)						
Baseline	245	257	.08	.06	.02	.39
FS I	245	256	.03	.01	.02	.05
FS II	245	255	.01	.01	.00	.96
FS III	245	257	.04	.04	.00	.92

In Table 4.5 we examine the intervention's impact on outcome indicators reflecting the access to formal financial products. Though statistically insignificant, the difference-in-difference estimates for the effects on access to financial products suggest a small positive impact on access to formal bank accounts. The overall effect shows a 2 percent increase in bank account holders. The overall impact on bank accounts opened specifically for business purposes is negative and insignificant. In Columns (3) and (4) however we observe a negative 3 percent impact at the first follow-up that is significant at the 10 percent level.

Table 4.5: Impact on Use of Financial Products

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Do you own a bank account? (0/1)						

Continued on next page...

... table 4.5 continued

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Overall treatment effect	.018	.016				
	.73	.74				
Treatment effect FS I			-.065	-.068	-.065	-.067
			.29	.26	.29	.27
Treatment effect FS II			.055	.051	.055	.053
			.36	.4	.36	.38
Treatment effect FS III					.063	.064
					.29	.29
Business bank account? (0/1)						
Overall treatment effect	-.033	-.031				
	.33	.34				
Treatment effect FS I			-.043	-.041	-.043	-.042
			.2	.22	.2	.22
Treatment effect FS II			-.034	-.033	-.034	-.033
			.34	.36	.34	.36
Treatment effect FS III					-.021	-.02
					.54	.55
Is your firm insured? (0/1)						
Overall treatment effect	-.01	-.009				
	.67	.71				
Treatment effect FS I			.006	.007	.006	.006
			.83	.79	.83	.81

Continued on next page...

... table 4.5 continued

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect FS II			-.019	-.017	-.019	-.017
			.45	.5	.45	.48
Treatment effect FS III					-.017	-.016
					.53	.57

Note: This table reports regression coefficients and p-values. The inference is based on heteroskedasticity-robust standard errors. The regressions reported in columns (1),(2),(5),(6) are based on all observations from all waves (pooled sample). The regressions reported in columns (3) and (4) exclude follow-up III observations. Individual covariates are included in regressions reported in columns (2),(4) and (6)

Neither the descriptive nor the empirical results show any evidence for an effect on the variable "Is your firm insured?". After baseline we observe a sharp drop in the share of insured ESAF clients in both groups. The drop being stronger in the control group results in a significant difference between controls and treated at follow-up I. To explain this temporary significant difference one could argue that both groups decreased the demand for insurance products and that the intervention cushioned this decrease for the treatment group. Regardless of the significant difference at follow-up I, the estimation results presented in Table 4.5, give no significant evidence that the intervention improved access or demand to formal financial products.

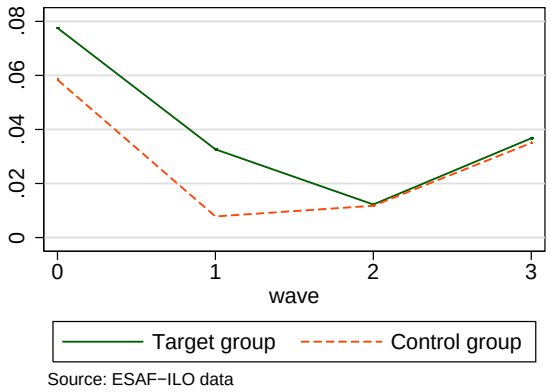

Figure 4.7: Have you insured your firm?

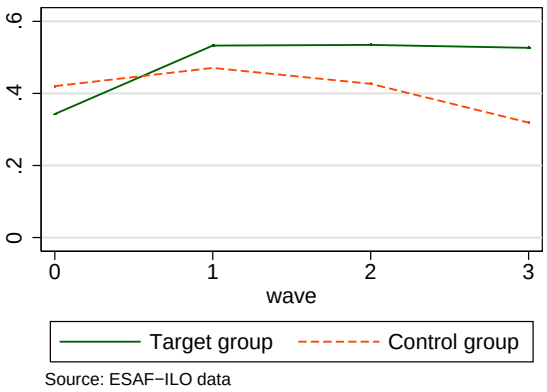

Figure 4.8: Member in any other network

Impact on Membership in Professional Networks and Access to Government Support Schemes

Leaving informality can enable microentrepreneurs to draw benefits from joining trade associations or applying for government support that require operating licenses[5]. We refer to trade associations as networks. Figure 4.8 shows how the share of clients who are members in such networks fares over time. We see a difference of eight percent between the treatment and the control group at baseline that is significant at the ten percent level and suggests that the control group is better connected at the outset (Table 4.6). At follow-up I the share of treatment clients who are members in other networks increased by 19 percent to 53 percent whereby the difference to the control group is still insignificant. At follow-up II membership in such networks is significantly higher by 10 percent in the treatment group.

Table 4.6 presents descriptive statistics on awareness about and access to government schemes. The p-values in the last column indicate that at any point in time there is a significant difference in the awareness about such schemes between treatment and control group. Over time we see that awareness more than triples in the

[5]Support programs include free health checks, business skill development training, access to government loans etc.

treatment group. From 11 percent at baseline it increases to 35 percent at follow-up III. Yet the number and the share of clients who benefited from such programmes is low in general. The share is even higher in the control group across all waves. But a mere glance at averages is deceiving here since the number of control clients for whom we observe this information is very low.

Table 4.6: Summary Statistics for Networks and Access to Government Schemes by Treatment Group

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
Member in other professional networks (0/1)						
Baseline	245	257	.34	.42	-.08	.07
FS I	244	257	.53	.47	.06	.17
FS II	245	225	.53	.43	.11	.02
Aware about Government support schemes (0/1)						
Baseline	245	257	.11	.07	.05	.06
FS I	245	255	.08	.01	.07	.00
FS II	245	255	.35	.04	.31	.00
Benefitted from Government support schemes (0/1)						
Baseline	28	17	.32	.47	-.15	.33
FS I	20	2	.75	1.0	-.25	.45
FS II	85	9	.28	.67	-.38	.02

For the last survey wave, follow-up III, more detailed questions were included in the survey asking whether the clients had actually applied for a public support scheme, and if yes, whether benefits were received. Table 4.7 shows that 17 percent of the treatment group compared to only 4 percent of the control group had applied for any scheme. The share of clients responding that they had received benefits from this scheme is equally high in both groups at about 40 percent.

Table 4.7: Summary Statistics on Access to Government Schemes by Treatment Group

	Mean		Mean	t-test		
Variable Names	Treated	Control	-difference	p-value		
Applied for support (last 2 yrs)						
FS III	244	256	.17	.04	.13	.00
Received benefits						
FS III	43	11	.42	.45	-.04	.83

4.4.2 Impact on Clients' Enterprise and Market Linkages Management Practices

In this section we analyze variables reflecting business performance and market integration. The intervention used training of basic book keeping principles to improve the clients' management practices. We see in Figure 4.9 that roughly 40 percent in both groups maintain books of accounts at baseline. While the treatment group has a slightly lower share of clients who maintain such books we observe that

the share surpasses the control group at follow-up I and is about ten percent higher at follow-up III.

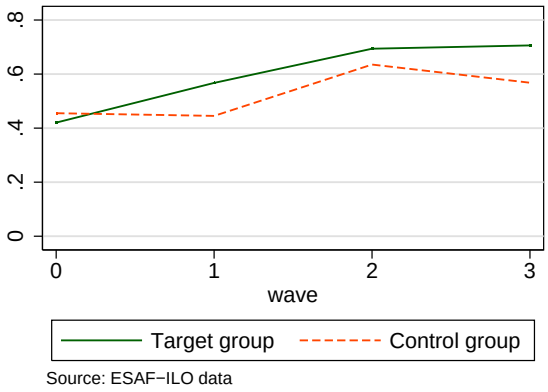

Figure 4.9: Maintains book of accounts?

Our estimations indicate that the intervention had a positive and significant impact on this measure of business management practice. The magnitude of the overall impact is fairly large with 14.6 percent when controlling for individual characteristics. The wave-specific impact is even at 17.9 percent at follow up III (see Table 4.8).

Table 4.8: Impact on Keeping Business Records

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Maintains book of accounts (0/1)						
Overall treatment effect	.141	.146				
	.01	.00				
Treatment effect FS I			.07	.071	.157	.161
			.19	.18	.01	.01
Treatment effect FS II			.007	.01	.093	.1
			.89	.84	.13	.11
Treatment effect FS III					.173	.179
					.00	.00

Note: This table reports regression coefficients and p-values.
The inference is based on heteroskedasticity-robust standard errors.
The regressions reported in columns (1),(2),(5),(6) are based on
all observations from all waves (pooled sample). The regressions
reported in columns (3) and (4) exclude follow-up III observations.
Individual covariates are included in regressions reported
in columns (2),(4) and (6)

Annual Turnover

Figure 4.10 presents the share of clients in the six different classes of annual turnover that were recorded during the interviews. For treatment and control group the average lies at around Rs. 70,000-80,000 in both groups at baseline. Though upward sloping, the average is fairly constant from baseline to follow-up III. In the

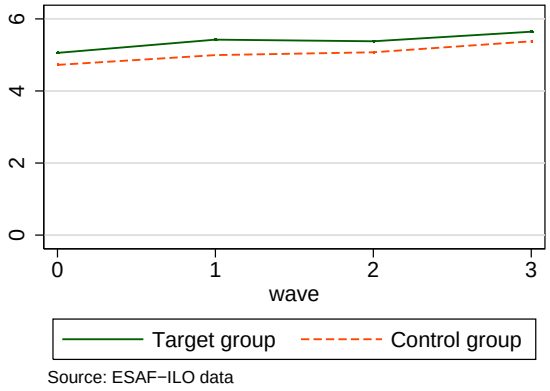

Figure 4.10: Annual turnover (in Rs.)

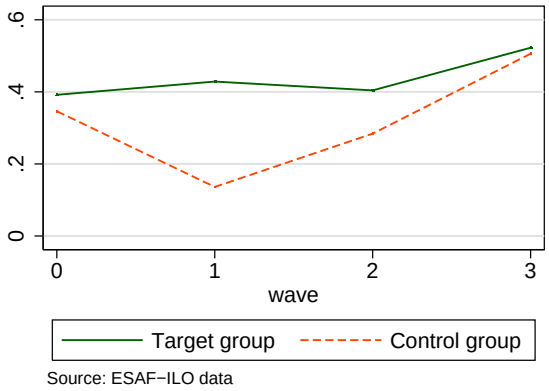

Figure 4.11: Turnover $\geq$ Rs. 100,000

treatment group annual turnover is constantly higher than in the control group which results mainly from a higher share of clients earning more than Rs. 100,000. In the treatment group this share is constantly at 40 percent and goes up to even 50 percent in the last wave. While there is no significant difference in turnover at baseline, we observe a slump in the highest income class for the control group in Figure 4.11 that leads to a significant difference between the two groups. The p-values displayed in Table 4.9 show significant differences at follow-up I in the highest income class.

Table 4.9: Summary Statistics of Annual Turnover (in Rs.) by Treatment Group

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
≤10000						
Baseline	245	257	.01	.02	-.01	.35
FS I	245	257	.02	.02	-.01	.57
FS II	245	257	.01	.00	.00	.54
FS III	245	257	.03	.07	-.03	.08
10001-30000						
Baseline	245	257	.1	.14	-.04	.19
FS I	245	257	.04	.06	-.01	.5
FS II	245	257	.05	.06	-.01	.66
FS III	245	257	.04	.06	-.01	.5
30001-50000						
Baseline	245	257	.2	.2	.00	.96
FS I	245	257	.12	.09	.03	.29
FS II	245	257	.09	.15	-.06	.03
FS III	245	257	.09	.09	.00	.89
50001-70000						
Baseline	245	257	.09	.12	-.03	.25
FS I	245	257	.15	.09	.05	.06
FS II	245	257	.19	.14	.04	.19

Continued on next page...

... table 4.9 continued

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
FS III	245	257	.1	.11	-.01	.59
70001-80000						
Baseline	245	257	.08	.11	-.02	.37
FS I	245	257	.08	.32	-.24	.00
FS II	245	257	.13	.2	-.07	.03
FS III	245	257	.07	.06	.01	.61
80001-100000						
Baseline	245	257	.12	.07	.06	.03
FS I	245	257	.16	.27	-.11	.00
FS II	245	257	.13	.16	-.03	.42
FS III	245	257	.14	.11	.04	.2
≥100001						
Baseline	245	257	.39	.35	.05	.29
FS I	245	257	.43	.14	.29	.00
FS II	245	257	.4	.28	.12	.00
FS III	245	257	.52	.51	.02	.71

The difference-in-difference estimates of the impact on annual turnover presented in Table 4.10 are all insignificant. The overall estimate of the impact on annual turnover is close to zero. Yet, we observe a significant impact on the share of clients in the highest turnover bracket, i.e. annual turnover above Rs. 100,000. The effect

is significant at the 10 percent or 5 percent significance level, depending on whether we control for individuals characteristics or not. The period specific estimations in Columns (3)-(6) reveal that this effect is driven by the control group's slump in turnover at follow-up I (see Figure 4.11 above). Given the assumption of parallel time trends holds these results suggest that in the absence of the intervention the treatment group would have suffered from a similar slump. But the estimates in the lowest row in Columns (5) and (6) show that at endline the impact is close to zero, negative and insignificant. It remains a puzzle what could have caused the sharp slump in the share of clients in the highest income bracket in the control group.

Table 4.10: Impact on Annual Turnover (in Rs.)

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Annual turnover (1-7)						
Overall treatment effect	.00	.019				
	1	.92				
Treatment effect FS I			.128	.135	.095	.11
			.5	.48	.68	.63
Treatment effect FS II			.005	.02	-.028	-.004
			.98	.91	.9	.98
Treatment effect FS III					-.066	-.05
					.79	.83
Turnover ≥100001						
Overall treatment effect	.097	.101				
	.05	.04				

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... table 4.10 continued

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect FS I			.261	.262	.247	.249
			.00	.00	.00	.00
Treatment effect FS II			.089	.092	.075	.079
			.09	.08	.22	.19
Treatment effect FS III					-.029	-.025
					.64	.68

Note: This table reports regression coefficients and p-values. The inference is based on heteroskedasticity-robust standard errors. The regressions reported in columns (1),(2),(5),(6) are based on all observations from all waves (pooled sample). The regressions reported in columns (3) and (4) exclude follow-up III observations. Individual covariates are included in regressions reported in columns (2),(4) and (6) Annual turnover is a categorical variable taking on values 1-7 according to the income brackets listed in table 4.9

Expansion Plans

We observe an a priori difference in expansion plans that emphasizes the need to apply an difference-in-difference evaluation strategy. In both groups, the clients selected for participation were to fulfill the condition to have plans to expand their business. Despite this being an overall condition for participation, we observe that among treatment group clients plans to expand are significantly more prevalent at baseline. Table 4.11 presents the descriptive analysis for expansion plans, and shows

that all through the period of observation, this share remains significantly higher in the treatment group.

Table 4.11: Summary Statistics of Plans to Expand Business by Treatment Group

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
Plans to expand						
Baseline	245	257	.8	.61	.19	.00
FS I	245	256	.82	.63	.19	.00
FS II	245	255	.73	.53	.21	.00
FS III	245	257	.72	.62	.1	.02

Investment in Machinery

As can be seen from Table 4.12 descriptively there is only a minor difference in the investment in new productive assets. In both groups around 40 percent stated at baseline that they bought new machinery. Both groups see a decline in such investments. At follow-up I and II we document a decrease to around 30 percent, which is even succeeded by a further decline to 20 percent at follow-up III. Also the cost of the acquired machinery is similar for both groups. Only at follow-up III we observe a significant difference in the average cost of the new machinery whereby it is the control group that had higher investments.

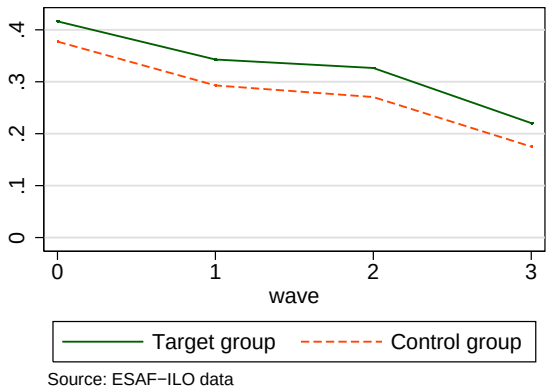

Figure 4.12: Bought any new machinery

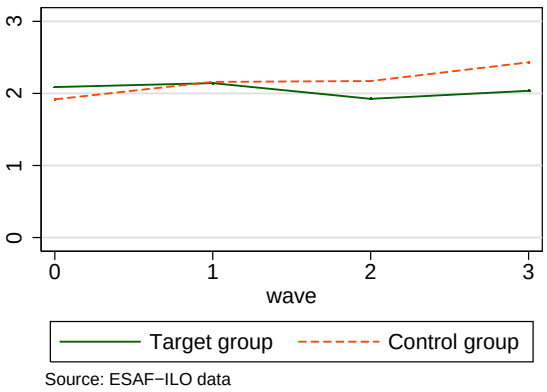

Figure 4.13: Cost of machinery

The cost at which the clients acquire the new machinery is on average at Rs. 5,000 to 25,000. As depicted in Figure 4.13 the development over time is constant and similar in both groups. One might thus conclude that the intervention has not had a significant impact on investment in new machines[6].

Table 4.12: Summary Statistics of Investment in Productive Assets by Treatment Group

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
Bought any new machinery						
Baseline	245	257	.42	.38	.04	.37
FS I	245	256	.34	.29	.05	.23
FS II	245	255	.33	.27	.06	.17
FS III	245	257	.22	.18	.05	.2
Cost of machinery						
Baseline	102	97	2.09	1.92	.17	.25
FS I	84	75	2.14	2.16	-.02	.92
FS II	80	64	1.93	2.17	-.25	.14
FS III	55	44	2.04	2.43	-.4	.05

[6]In Figure 4.13 the vertical axis is labeled with 1 = 'below Rs. 5000', 2 = 'Rs. 5000-25,000', 3 = 'Rs. 25,000-50,000'.

Employment

Employment is an essential outcome indicator to assess the growth of the clients' enterprise. Figure 4.14 displays the share of ESAF clients who have any employee in their enterprise. Figure 4.15 shows the average number of employees in both groups for those who have employees. The share of ESAF-clients who hire employees is far lower in the control group with only 26 percent as compared the treatment group where it is at 41 percent. Table 4.13 shows that the baseline difference is highly significant. At the first follow-up interview employment in the treatment group declined by 22 percent, such that both groups have a share of 19 percent of clients with any employees. At follow-up II employment picks up again in both groups, and we observe very small insignificant differences only.

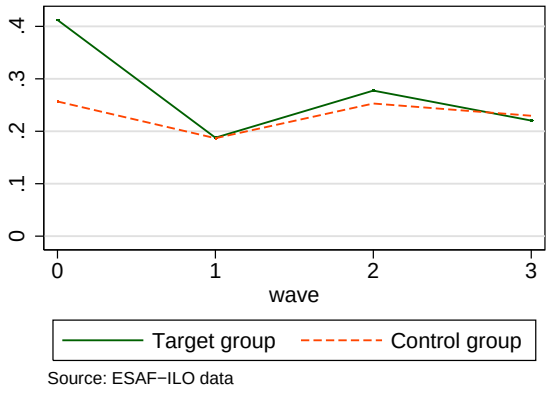

Figure 4.14: Do you have employees?

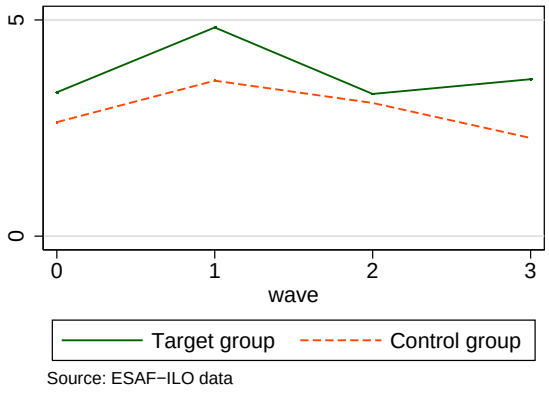

Figure 4.15: Number of employees

The size of the enterprise, measured by the number of employees, is slightly higher in the treatment group at all points in time whereby it is only at follow-up III where the difference is significant at the 10 percent level.

Table 4.13: Summary Statistics on Employment in Client's Enterprise by Treatment Group

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
Do you have employees? (0/1)						
Baseline	245	257	.41	.26	.16	.00
FS I	245	257	.19	.19	.00	.98
FS II	245	257	.28	.25	.02	.53
FS III	245	257	.22	.23	-.01	.81
Number of employees						
Baseline	101	66	3.33	2.64	.69	.18
FS I	46	47	4.83	3.6	1.23	.46
FS II	45	62	3.29	3.08	.21	.8
FS III	54	59	3.63	2.27	1.36	.07

The estimates of the impact on the share of clients who have any employees are negative and significant. The descriptive results indicate that this effect is driven by the strong decrease in the treatment group at follow-up I. Several interpretations for this phenomenon can be offered. (1) Formalization might give incentives to reduce the number of employees (to zero). (2) The number of employees might have remained the same but formalized respondents could feel uneasy to report unregistered employees in the interview. The former could be considered a negative side-effect of formalization. Among those that hire, the number of employees was not affected.

Table 4.14: Impact on Employment in Client's Enterprise

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Do you have employees? (0/1)						
Overall treatment effect	-.15	-.155				
	.00	.00				
Treatment effect FS I			-.154	-.159	-.154	-.16
			.00	.00	.00	.00
Treatment effect FS II			-.131	-.135	-.131	-.136
			.02	.02	.02	.02
Treatment effect FS III					-.165	-.168
					.00	.00
Number of employees						
Overall treatment effect	.243	.007				
	.75	.99				
Treatment effect FS I			.54	-.235	.54	-.124
			.75	.89	.75	.94
Treatment effect FS II			-.482	-.948	-.482	-.806
			.58	.29	.58	.36
Treatment effect FS III					.668	.834
					.45	.36

Note: This table reports regression coefficients and p-values. The inference is based on heteroskedasticity-robust standard errors. The regressions reported in columns (1),(2),(5),(6) are based on all observations from all waves (pooled sample). The regressions reported in columns (3) and (4) exclude follow-up III observations. Individual covariates are included in regressions reported in columns (2),(4) and (6)

Access to Electricity

Ease of access to electricity is one advantage that formalization supposedly offers. Through special programmes microentrepreneurs are offered reductions on their electricity bills. In our sample the intervention did not make a difference. As shown in Figure 4.16 the availability of electricity in the business at baseline is slightly lower in the treatment group with a share of 59 percent compared to 67 percent in the control group. While the control group share remains constant it increases in the treatment branches by 6 percent. Though statistically insignificant, at follow-up III we observe even a slightly positive difference in favor for the treatment group (Table 4.15). Despite the slight increase the overall impact estimate is close to zero and insignificant (Table 4.16).

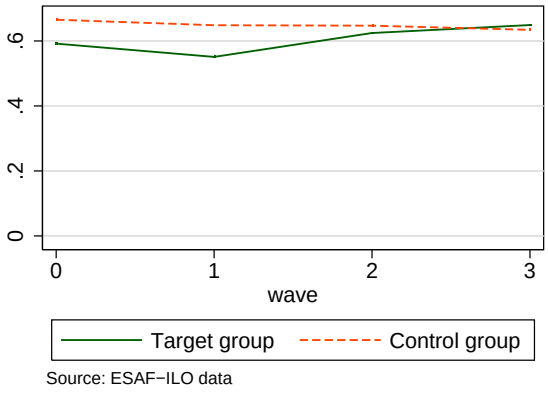

Figure 4.16: Business has electricity?

Table 4.15: Summary Statistics on Accesss to Electricity

by Treatment Group

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
Business has electricity (0/1)						
Baseline	245	257	.59	.67	-.07	.09
FS I	245	256	.55	.65	-.1	.03
FS II	245	255	.62	.65	-.02	.6
FS III	245	257	.65	.63	.01	.73

Table 4.16: Impact on Access to Electricity

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Business has electricity (0/1)						
Overall treatment effect	.038	.04				
	.44	.42				
Treatment effect FS I			-.024	-.025	-.024	-.022
			.7	.68	.7	.72
Treatment effect FS II			.051	.049	.051	.053
			.4	.42	.4	.38
Treatment effect FS III					.088	.088
					.15	.15

Note: This table reports regression coefficients and p-values. The inference is based on heteroskedasticity-robust standard errors. The regressions reported in columns (1),(2),(5),(6) are based on all observations from all waves (pooled sample). The regressions reported in columns (3) and (4) exclude follow-up III observations. Individual covariates are included in regressions reported in columns (2),(4) and (6)

Demand, Advertisement and Market Linkages

Figure 4.17 and Figure 4.18 present the share of ESAF clients who report that their product is in high or low demand, respectively. The share of clients who report that their product is in high demand is at roughly 55 percent at baseline for both groups. Both groups see a decline in this share at follow-up I. This decline is stronger for the treatment group. However, after follow-up I, the indicator remains constant while it further declines in the control group. At follow-up III the share of clients who have high demand for their product is 14 percent higher in the treatment group (Table 4.17).

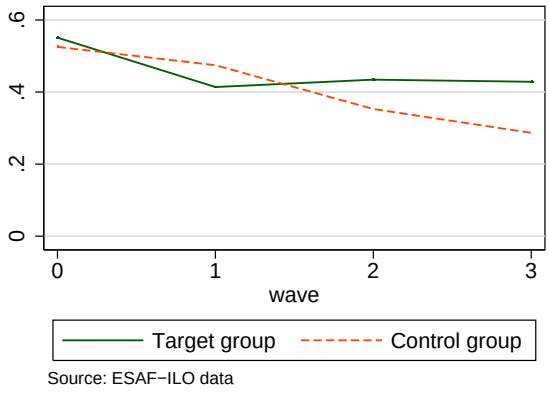

Figure 4.17: Is your product in high demand?

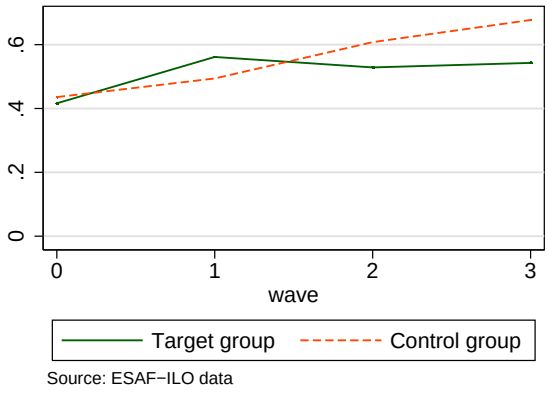

Figure 4.18: Is your product in low demand?

The difference-in-difference estimates of the overall effect of the intervention on the indicator variable "product is in high demand" is insignificant (Table 4.18, Columns (1) and (2)). The effect for the last follow-up is significant at the 10 percent level (columns (5) and (6)), i.e. treatment group clients have an 11 percent higher probability to state that there is high demand for their products. We observe the same pattern but with reversed signs for low demand. The overall effect is insignif-

icant but the estimate for the last follow-up is significant and amounts to an 11 percent decrease in the probability to state that demand is low.

Table 4.17: Summary Statistics on Demand and Advertisement by Treatment Group

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
Is your product in high demand? (0/1)						
Baseline	245	257	.55	.53	.03	.56
FS I	244	255	.41	.47	-.06	.17
FS II	244	255	.43	.35	.08	.06
FS III	245	251	.43	.29	.14	.00
Is your product in low demand? (0/1)						
Baseline	245	257	.42	.44	-.02	.66
FS I	244	255	.56	.49	.07	.13
FS II	244	255	.53	.61	-.08	.07
FS III	245	251	.54	.68	-.13	.00
Do you advertise? (0/1)						
Baseline	245	257	.27	.22	.05	.18
FS I	245	256	.38	.26	.12	.00
FS II	245	255	.53	.31	.22	.00
FS III	245	257	.6	.49	.11	.01

In part the observed pattern might be directly related to advertisement activities. Figure 4.19 shows that the share of advertising clients increased more strongly in the treatment group. At the baseline it was already higher for the treatment group with 27 percent compared to 22 percent in the control group but this difference was not significant (Table 4.17). At follow-up I, however, the mean difference amounts to 12 percent and is significant at the one percent level. At the second follow-up the difference increases to 22 percent and then drops again at follow-up III to 11 percent. At follow-up III the majority of 60 percent of the treatment group claims to engage in advertising.

The estimations presented in Table 4.18 provide evidence for the causal effect of the intervention on advertisement activities. This outcome is of special interest since the training conducted in the treatment group also included marketing and advertising. Any effect here can be attributed directly to the formalization activities. The estimate of the overall effect is 9.9 percent and has a p-value indicating significance at the 5 percent level.

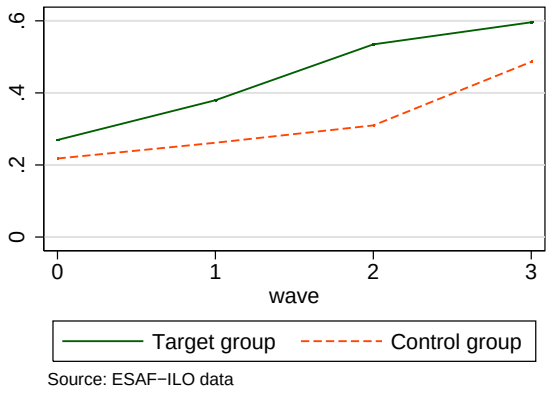

Figure 4.19: Do you advertise?

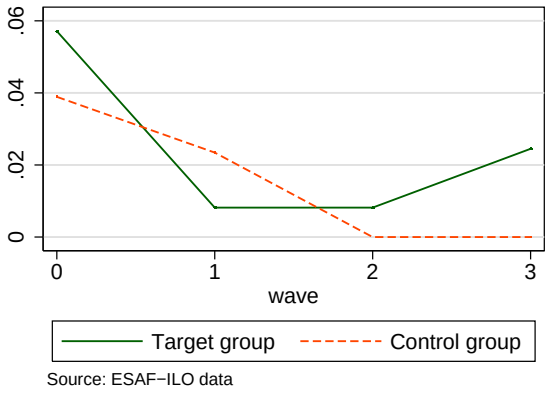

Figure 4.20: Do you export?

Figure 4.20 shows the share of clients who export their products. Almost 6 percent of treatment group clients export their products at baseline. In the control group

the share is slightly lower at 4 percent. In both groups product exports decline at follow-up I but stronger so in the treatment branches. However, the treatment group sees an increase in exports after follow-up II while it drops to zero in the control group. Though not depicted here, the estimates for the impact on exports are all insignificant and close to zero.

Table 4.18: Impact on Demand and Advertisement

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Is your product in high demand? (0/1)						
Overall treatment effect	.028	.023				
	.58	.65				
Treatment effect FS I			-.086	-.09	-.086	-.09
			.17	.15	.17	.15
Treatment effect FS II			.056	.051	.056	.051
			.37	.41	.37	.4
Treatment effect FS III					.116	.109
					.06	.07
Is your product in low demand? (0/1)						
Overall treatment effect	-.029	-.024				
	.57	.64				
Treatment effect FS I			.087	.091	.087	.091
			.17	.14	.17	.14
Treatment effect FS II			-.06	-.055	-.06	-.055
			.34	.37	.34	.37

Continued on next page...

... table 4.18 continued

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect FS III					-.115	-.108
					.06	.07
Do you advertise? (0/1)						
Overall treatment effect	.099	.098				
	.03	.03				
Treatment effect FS I			.066	.066	.066	.068
			.24	.24	.24	.23
Treatment effect FS II			.173	.171	.173	.172
			.00	.00	.00	.00
Treatment effect FS III					.058	.053
					.32	.36

Note: This table reports regression coefficients and p-values. The inference is based on heteroskedasticity-robust standard errors. The regressions reported in columns (1),(2),(5),(6) are based on all observations from all waves (pooled sample). The regressions reported in columns (3) and (4) exclude follow-up III observations. Individual covariates are included in regressions reported in columns (2),(4) and (6)

4.4.3 Impact on Household Income

Monthly Household Income

Figure 4.21 presents monthly household income. For both groups the graphs have positive slopes, whereby the control group peaks at follow-up I and the treatment group has overall a higher income growth from baseline to follow-up III. As Figure

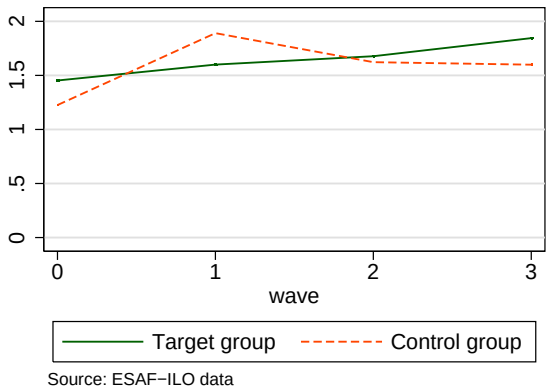

Figure 4.21: Monthly household income

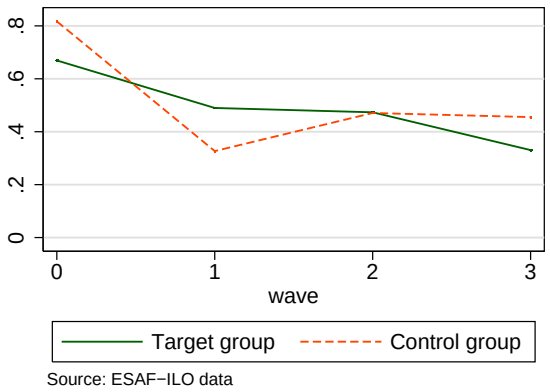

Figure 4.22: Income <10.000

4.22 shows, the share of clients in the lowest income class, i.e. income < Rs. 10,000, declines in both groups between baseline and follow-up I, but the decrease is steeper in the control group. This decrease is accompanied by increases in the two other income brackets. Especially, for the income bracket "Rs. 20,001 - 30,000" we observe a large difference in the increase between treatment and control clients. For the control group the share of clients in this income class increases from virtually zero to 20 percent (Figure 4.24).

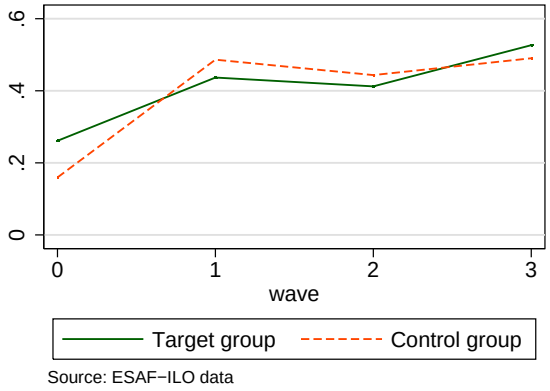

Figure 4.23: Income 10001 - 20000

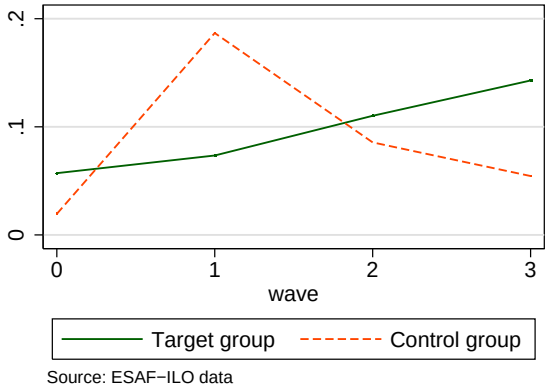

Figure 4.24: Income 20001 - 30000

Table 4.19 allows a more thorough look at the income situation. Among treatment clients a share of 67 percent has monthly household income below Rs. 10,000 at

baseline. Among control clients this share is significantly higher at 82 percent. Both groups see substantial shifts from the lowest to the next income group (Rs. 10,001-20,000). In the control group the share of clients in the lowest income group decreases by 50 percent already at follow-up I. While in the treatment group the shift is mostly to the second income class (Rs. 10,001 - 20,000 Rs.) we observe that in the control group the share of clients with income between Rs. 20,001 - 30,000 increases substantially, too.

At follow-up III, we observe significant differences in income group three (Rs. 20,001 - 30,000) and four (Rs. 30,001 - 40,000), both in favor for the treatment group. Yet, the share of clients with monthly household income higher than Rs. 30,000 is only 2 percent in the treatment group.

Table 4.19: Summary Statistics on Monthly Household Income by Treatment Group

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
					Monthly Household Income	
≤10000						
Baseline	245	257	.67	.82	-.15	.00
FS I	245	257	.49	.33	.16	.00
FS II	245	257	.47	.47	.00	.95
FS III	245	257	.33	.46	-.12	.00
10001-20000						
Baseline	245	257	.26	.16	.1	.01

Continued on next page...

... table 4.19 continued

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
FS I	245	257	.44	.49	-.05	.27
FS II	245	257	.41	.44	-.03	.48
FS III	245	257	.53	.49	.04	.42
20001-30000						
Baseline	245	257	.04	.01	.03	.02
FS I	245	257	.06	.16	-.1	.00
FS II	245	257	.09	.08	.02	.52
FS III	245	257	.11	.05	.06	.02
30001-40000						
Baseline	245	257	.01	.01	.00	.96
FS I	245	257	.01	.02	-.01	.45
FS II	245	257	.01	.01	.00	.96
FS III	245	257	.02	.00	.02	.01
40001-50000						
Baseline	245	257	.00	.00	.00	.31
FS I	245	257	.00	.01	.00	.59
FS II	245	257	.01	.00	.01	.15
FS III	245	257	.00	.00	.00	.31
$\geq$50001						
Baseline	245	257	.01	.00	.01	.29
FS I	245	257	.00	.00	.00	

Continued on next page...

... table 4.19 continued

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
FS II	245	257	.00	.00	.00	.31
FS III	245	257	.00	.00	.00	

Table 4.20 assesses whether the shift from the lowest income group to higher income groups is constrained to certain economic activities. In both groups, all clients with agricultural businesses are in the lowest income group at baseline and in both groups this share decreases substatially at follow-up I. Much more however in the treatment group where only 29 percent of the clients in agriculture remain in the lowest income group. In the treatment group we observe a stark decline in low incomes from 100 to 29 percent for clients with agricultural activities. This might indicate that seasonal effects drive the upward shift in incomes. In the control group the share of agricultural clients in the lowest income group decreases as well but the change is much weaker. In all other activities in both groups the share in the lowest income category declines, too, In the control group it is largest for the trade and service activities, which are likely to have much less seasonal fluctuation than agriculture.

We present the difference-in-difference estimates for the impact of the intervention on monthly household income in Table 4.21. The overall effect of the intervention in Columns (1) and (2) is negative and significant. This would suggest that the intervention has caused a decrease of incomes of households in the treatment group. A glance at columns (3) to (6) reveals that the drop in incomes at follow-up I drives

Table 4.20: Share in Lowest Income Class (<10,000) by Type of Activity and Treatment Group

	Control				Treatment			
Survey wave	Baseline	FS I	FS II	FS III	Baseline	FS I	FS II	FS III
Agriculture/Animals	1.0	.83	.6	.67	1.0	.29	.5	.00
Hotel/Restaurant	.79	.45	.46	.52	.57	.38	.39	.28
Mobile trading	.8	.39	.29	.44	.88	.42	.7	.43
Production	.8	.46	.55	.58	.55	.44	.31	.27
Services	.79	.31	.5	.53	.75	.46	.55	.44
Trade/Commerce	.81	.23	.47	.35	.67	.59	.44	.36
Total	.82	.33	.48	.46	.67	.49	.47	.335

this result. Also the coefficients for follow-up II are negative though lower in size and in significance. Since the increase in incomes is observed in the control group for all professions the explanation of seasonal fluctuation would only be a valid one if all professions were subject to these fluctuations.

The treatment group's improved capacities to keep records of their business and household finances offers an alternative explanation for the changes in income (see Section 4.4.2). Improved and more factual book-keeping, e.g. through keeping records of expenses, costs, purchases, profits, can bring out more realistic figures. In the control group, on the other hand, clients continue the usual procedure of keeping records. Yet, the changes in income in the control group refute this explanation.

Table 4.21: Impact on Monthly Household Income

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Monthly Household Income						
Overall treatment effect	-.224	-.22				
	.00	.00				

Continued on next page...

... table 4.21 continued

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect FS I			-.518	-.509	-.518	-.51
			.00	.00	.00	.00
Treatment effect FS II			-.172	-.161	-.172	-.162
			.06	.06	.06	.06
Treatment effect FS III					.018	.013
					.83	.87

Note: This table reports regression coefficients and p-values. The inference is based on heteroskedasticity-robust standard errors. The regressions reported in columns (1),(2),(5),(6) are based on all observations from all waves (pooled sample). The regressions reported in columns (3) and (4) exclude follow-up III observations. Individual covariates are included in regressions reported in columns (2),(4) and (6)

Main Source of Household Income

The household enterprise plays an important role for the majority of clients in both groups (Figure 4.25). Over 70 percent generate the main income through a household enterprise, followed by 30 percent that do so through casual wage labor. In the treatment group 69 percent of clients at baseline generate the main source of income from the household enterprise and this increases to 94 percent at follow-up I (Table 4.22). At follow-up II we observe a slight decrease to 91 percent and to 86 percent at follow-up III but still the share remains constantly higher than in the control group.

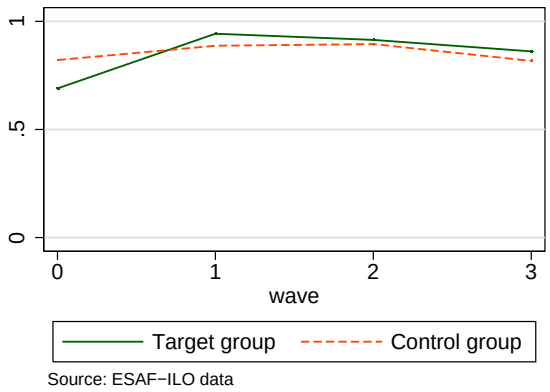

Figure 4.25: Income bracket: 10001 - 20000

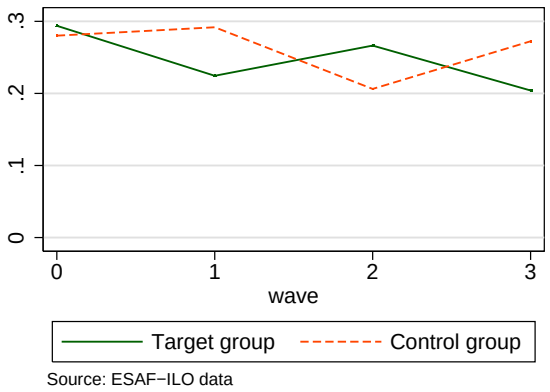

Figure 4.26: Income bracket: 20001 - 30000

The descriptive results for the main source of income are presented in Table 4.22. In both groups, around ten percent of clients generate their main income through regular wage labor. The shares for commissioned/contracted work as well as for remittances are below five percent in both groups. For the latter we observe a significant difference at follow-up III. The effect size is small though at 2.5 percent overall and 4.1 percent in the last survey wave.

Table 4.22: Summary Statistics on Main Income Source of Household by Treatment Group

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
Main income source						
Casual Wage Labour (0/1)						
Baseline	245	257	.29	.28	.01	.73
FS I	245	257	.22	.29	-.07	.09

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... table 4.22 continued

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
FS II	244	257	.27	.21	.06	.11
FS III	245	257	.2	.27	-.07	.07
Household enterprise (0/1)						
Baseline	245	257	.69	.82	-.13	.00
FS I	245	257	.94	.89	.06	.03
FS II	245	257	.91	.89	.02	.46
FS III	245	257	.86	.82	.04	.18
Regular salary (0/1)						
Baseline	245	257	.09	.11	-.02	.57
FS I	245	257	.13	.08	.05	.06
FS II	245	257	.14	.11	.03	.38
FS III	245	257	.11	.11	.00	.97
Comissioned/Contract work (0/1)						
Baseline	245	257	.03	.01	.02	.11
FS I	245	257	.03	.02	.02	.21
FS II	245	257	.02	.03	-.01	.45
FS III	245	257	.04	.05	-.01	.45
Remittances (0/1)						
Baseline	245	257	.01	.02	-.01	.45
FS I	245	257	.04	.02	.02	.14
FS II	245	257	.02	.02	.00	.95

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... table 4.22 continued

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
FS III	245	257	.05	.02	.03	.03

In Table 4.23 the overall impact in Columns (1) and (2) tells us that as a result of the intervention the share of clients who state that the household enterprise is the main income source increased by 17 percent. The effects for follow-up I and follow-up II in Columns (3) and (4) are all positive and significant as well. One might conclude that the clients' perception of the importance that her enterprise has for the household has changed due to the formalization intervention.

Regarding the other sources of income, the relevance of casual wage labor was not affected but we observe a small and significant impact of 3 percent on the importance of remittances. Specifications (5) and (6) indicate that this effect appears only at follow-up III. The causal interpretation of this effect suggests that the intervention has (indirectly) increased the dependence or necessity of remittances in the treatment group.

Table 4.23: Impact on "What is the main income source of the household?"

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Household enterprise (0/1)						
Overall treatment effect	.171	.169				

Continued on next page...

... table 4.23 continued

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
	.00	.00				
Treatment effect FS I			.187	.185	.187	.188
			.00	.00	.00	.00
Treatment effect FS II			.151	.147	.151	.149
			.00	.00	.00	.00
Treatment effect FS III					.175	.171
					.00	.00
Casual wage labour (0/1)						
Overall treatment effect	-.039	-.033				
	.4	.47				
Treatment effect FS I			-.081	-.076	-.081	-.077
			.15	.17	.15	.16
Treatment effect FS II			.046	.056	.046	.055
			.4	.31	.4	.32
Treatment effect FS III					-.082	-.076
					.14	.16
Remittances (0/1)						
Overall treatment effect	.026	.025				
	.04	.04				
Treatment effect FS I			.029	.028	.029	.027
			.1	.1	.1	.11
Treatment effect FS II			.008	.008	.008	.007

Continued on next page...

... table 4.23 continued

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
			.58	.6	.58	.62
Treatment effect FS III					.041	.041
					.03	.03

Note: This table reports regression coefficients and p-values. The inference is based on heteroskedasticity-robust standard errors. The regressions reported in columns (1),(2),(5),(6) are based on all observations from all waves (pooled sample). The regressions reported in columns (3) and (4) exclude follow-up III observations. Individual covariates are included in regressions reported in columns (2),(4) and (6)

4.4.4 Impact on Client's Loan Size and Repayment

As can be seen from figure 4.27 and Table 4.24 the loan size differs only by a small amount (Rs. 513) at the start of the intervention. The amount of the last loan remains almost constant in the treatment group until follow-up survey II and only slightly picks up towards follow-up survey III. The control group sees more fluctuation in this outcome. At first, loan size decreases by about Rs. 1,700 and then, at follow-up survey II, it increases by Rs. 5,400. The decrease we observe in follow-up survey III is small again.

The direct comparison between treatment and control group for this outcome is difficult. As mentioned, at baseline the difference is only small and insignificant. At follow-up survey I we observe that on average the treatment group takes out significantly higher loans. The averages for follow-up survey II show a highly significant

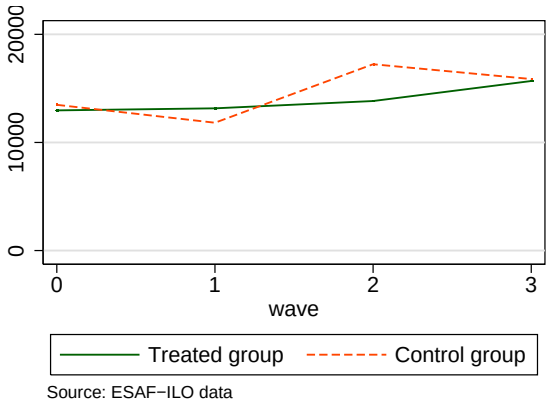

Figure 4.27: Amount of last ESAF loan

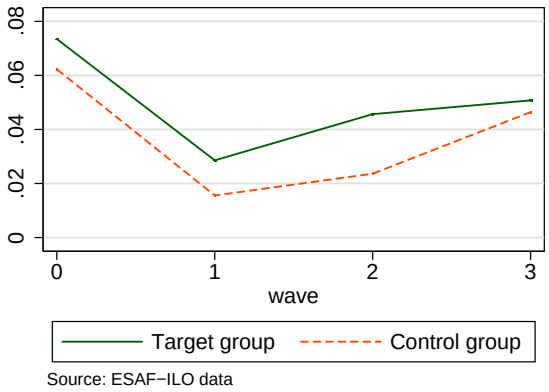

Figure 4.28: Repayment Difficulties

difference, with the control group taking out loans that are on average Rs. 3400 higher. Cautiousness is required when interpreting this result as in the treatment group at follow-up II and III about 100 clients state to have either "Loan expired" or "Loan closed".

Table 4.24: Summary Statistics on Clients' Relation to ESAF by Treatment Group

	Sample Size		Mean	Mean	*t*-test
Variable Names	Treated	Control	Treated	Control	-difference	*p*-value
Amount of last EASF loan (0/1)						
Baseline	245	257	12967.3	13480.5	-513.2	.32
FS I	245	256	13156.9	11826.7	1330.2	.08
FS II	144	245	13833.3	17236.7	-3403.4	.00
FS III	197	232	15685.8	15866.4	-180.6	.83
No ESAF loan (0/1)						
Baseline	245	257	.00	.00	.00	

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... table 4.24 continued

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
FS I	245	257	.00	.00	.00	.33
FS II	245	257	.49	.05	.44	.00
FS III	245	257	.21	.12	.09	.01
Difficulties repaying loan (0/1)						
Baseline	245	257	.07	.06	.01	.62
FS I	245	257	.03	.02	.01	.32
FS II	219	254	.05	.02	.02	.19
FS III	197	237	.05	.05	.00	.83
Unforseen expenses (0/1)						
Baseline	245	257	.43	.53	-.1	.02
FS I	245	257	.37	.24	.13	.00
FS II	245	256	.3	.45	-.15	.00
FS III	244	256	.34	.41	-.08	.07

At follow-up survey III the significant difference disappears and both groups have loans of on average Rs. 16,000. The problem is less pronounced here with only 50 observations having missing value. Still missing values in the treatment group come exclusively from Tamil Nadu. The variable *No ESAF loan* captures whether the individual has either missing value for *amount last loan* or zero. We observe that at baseline and follow-up survey I every respondent stated that she had a positive loan. At follow-up survey II nearly half of the treatment group has no last loan. At

follow-up III this share is at 20 percent. One explanation for the high occurrence of missing values is that clients who formalize become eligible for loans from formal banks. This in addition to the argument that they might require loans of a size that ESAF cannot deliver might explain the phenomenon starting at follow-up survey II. However, a comparison of means within the treatment group at follow-up survey II shows that among those who registered the share of *no ESAF loan* is lower than among those who did not register. A t-test also provides significant evidence that the share of clients without loan is higher among unregistered clients.

Table 4.25: Impact on Loan Behavior

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Last loan amount						
Overall treatment effect	12.68	87.5				
	.99	.9				
Treatment effect FS I			1843.44	1856.91	1843.44	1827.27
			.05	.03	.04	.04
Treatment effect FS II			-2890.2	-2513.32	-2890.2	-2625.04
			.00	.01	.00	.00
Treatment effect FS III					332.61	310.03
					.75	.76
Difficulties in repaying loan (0/1)						
Overall treatment effect	.00	.00				
	.93	.95				
Treatment effect FS I			.00	.00	.00	.00

Continued on next page...

... table 4.25 continued

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
			.95	.97	.95	.96
Treatment effect FS II			.01	.01	.01	.01
			.7	.74	.7	.73
Treatment effect FS III					-.01	-.01
					.82	.82

Note: This table reports regression coefficients and p-values. The inference is based on heteroskedasticity-robust standard errors. The regressions reported in columns (1),(2),(5),(6) are based on all observations from all waves (pooled sample). The regressions reported in columns (3) and (4) exclude follow-up III observations. Individual covariates are included in regressions reported in columns (2),(4) and (6)

When we consider the estimations for the impact on loan size (table 4.25) we need to take into account the prevalence of missing values. The overall effect on loan size is insignificant which comes with no surprise since the differences at baseline and follow-up survey III are only small. The effect at follow-up survey I, however, is positive and significant when estimated by model 3, i.e. as a result of the intervention clients from treatment branches took out loans that were about Rs. 1,800 higher than in the control group. At follow-up survey II the effect reverses, with control clients having loans that are about Rs. 3,000 higher. At follow-up survey III the impact is at only Rs. 800 and insignificant. The fact that the sign switches for different points in time becomes apparent when looking at Figure 4.27. It shows that the amount of the last loan fluctuates in opposing directions at follow-up survey I and II.

Figure 4.28 shows that repayment difficulties are slightly more prevalent in the treatment branches. However, we learn from Table 4.24 that the difference is not significant at any point of investigation. The difference-in-difference estimates for repayment difficulties are all close to zero indicating that the capacity to repay a loan was not affected.

4.5 Conclusion

The assessment of ESAFs formalization intervention shows that for microfinance clients with well established informal enterprises formalization is a welcome option. We observe that mere distribution of information on the benefits and processes coupled with targeted support for the registration leads to substantial rates of formalization. This suggests that the direct costs of formalization can be borne by the entrepreneurs. The increase in registrations persists over the period of investigation which leads us to subsume that the consequential costs of formalization e.g. through payment of taxes do not exceed the benefits that the entrepreneurs draw from the change in status.

What remains unclear is what are the benefits that the entrepreneurs draw exactly from formalization. Beyond increased access to government support schemes we observe little evidence that points to an improved situation in the treatment group. Hard business indicators, such as turnover, did not change. Given the high registration rates and concurrent tax obligation, the latter suggests rather that profits from the business activity must have decreased. The increased reliance on remittances that we observe might compensate for the assumed reduction in profits.

On the other hand, the positive effects on 'soft' outcomes such as the perception of the business or, also on the entrepreneurs subjective assessment on product demand,

show that formalization nurtured aspirations in the treatment group. A follow-up to observe the long-term sustainability could possible show whether formalization succeeded in satisfying these aspirations.

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CHAPTER V

The Impact of a Formalization Intervention in Burkina Faso

5.1 Introduction

The informal sector's importance for African economies is no longer an issue of dispute. Its high relevance for income generation is well established (Benjamin et al., 2012) and so is the fact that it is not a transitory phenomenon (Ihrig and Moe, 2004). The literature now recognizes also the sector's heterogeneity, as it encompasses a wide range of different activities, different industries, and harbors enterprises varying substantially in size and productivity (Grimm et al., 2012; Trager, 1987).

Yet, a continuing discussion deals with the question how to address issues that enterprises face in the informal sector. Despite the fact that the sector is recognized as a permanent feature of the economy, formalization remains an answer to this question. The present paper assesses an intervention that fosters formalization among the clients of a large microfinance institution in Burkina Faso. The implementing agency is the *Réseau des Caisses populaires du Burkina* (RCPB), a savings and credit cooperative with over 151,000 active borrowers and 852,000 depositors[1][2].

[1]Information available on `http://www.mixmarket.org/mfi/rcpb`.

[2]RCPB has 159 branches in 44 out of the 45 provinces of Burkina and is the largest micro finance provider in the country.

According to RCPB the costs accruing from formalization and the lack of information about it are the main obstacles keeping its clients from registering their activities. The agency claims that lack of correct information leads clients to form unrealistic perceptions of formalization. This leads clients who could otherwise benefit from formality to remain informal[3] In order to increase the rate of formalization, RCPB tackles ignorance and the distorted picture that its clients have about formality.

Three main goals are at the heart of RCPBs efforts to support formalization among its clients. The first goal is to increase security and stability, which might both accrue from registration as it liberates clients from risks related to informality: the threat of business closure from fiscal authorities, the risk of penalty payments for violating regulations, and the burden of repeated bribe payments that can consume large parts of the business revenue and potentially exceed the volume of tax liabilities that registered enterprises face. Especially, successful informal enterprises run these risks as they are most prone to attract the inspectors' attention (Djankov et al., 2003).

Secondly, it is only after formalization that enterprises can grant their employees access to the "Caisse Nationale de Securite Sociale" (CNSS), the social security system in Burkina Faso[4]. Thirdly, enrollment with the local authorities gives increased pressure on RCPBs clients to keep records of their business activities which, by itself, can be expected to have positive effects on business management.

The literature on the informal sector in West Africa offers further reasons in favor for formalization. Böhme and Thiele (2012) provide evidence that the infor-

[3]For a more general discussion of the reasons of informality and the relevant literature see Chapter IV.

[4]The CNSS covers costs related to sickness, accidents and maternity and provides an old age pension scheme (see `http://www.cnss.bf/index_eng.html`.)

mal sector in the region faces demand constraints. They argue that increases in income lead people to shift consumption from informal products to products from the formal sector. E.g., informal distribution channels turn to deliver formal rather than informal products. Grimm et al. (2012) coin the term of *constrained gazelles* that refers to informal enterprises with high profitability but little access to capital. Those RCPB clients falling under this category might unleash entrepreneurial potential after formalization as RCPB access to larger business loans on it.

Our analysis of the pilot intervention shows that formalization can be fostered in the targeted segment. However, we observe that entrepreneurs revise their decision quite quickly and return to informality.

The paper continuous with a description of the intervention in the next section. We present the data and the evaluation strategy in section 3 and the results in section 4. In section 5 we discuss the results.

5.2 The Context and Intervention

Regarding the level of formalization RCPB categorizes its clients in three groups[5]. The first group consists of unregistered enterprises that do not pay taxes, nor any other official fees. This group of entrepreneurs is not enrolled with the CNSS, nor is it registered with the *Maison de l'Entreprise du Burkina Faso* (MEBF) that issues a unique financial identification (UFI) number and requires registered entrepreneurs to submit financial statements. The second group is labeled informal sector enterprises. These are semi-formalized enterprises, recognized by the MEBF, holding a UFI number and possibly holding an Informal Sector Card or a Commercial Card. These cards are issued by the *Centre de Formalités des Entreprises* (CEFOR). They

[5]For a discussion of different definitions of the informal sector see Lubell (1991).

give proof of legal recognition but are no titles yet for formal activity. Clients in this group pay taxes on profits and they pay for patents. They are not enrolled with the CNSS.

Formal enterprises, the third group, hold a Commercial Card, have a UFI number and, in addition to the taxes on their profits and charges for patents, they pay a value-added-tax on goods and services and taxes on commercial or industrial benefits. The tax rates they face are higher than those for the second group. Further, affiliation to this category requires that the enterprise is enrolled with the CNSS and pays the contribution for at least some of the employees[6].

The intervention has three different features. First, the treatment group clients were invited to a sensitization workshop. At the workshop formalization procedures, the risks of informality, and the advantages of formalization were illustrated. Second, a training workshop was conducted that treated topics like stock management, costs management, accounting, financial budgeting, the services of the CNSS and formalization procedures. Regarding the latter, it was explained in detail what judicial form and fiscal regime the clients could choose and what administrative issues the formal creation of an enterprise requires. These topics were identified by RCPB as substantial knowledge gaps among the clients.

The first two components were implemented in November 2010 and only 209 of the 300 treatment clients participated. The third component provided additional incentives for formalization. A competition was announced where good formalization practice was to be rewarded. The entrepreneurs were to be evaluated according

[6]Registering a formal enterprise requires the entrepreneur to subscribe with CEFOR. The procedure costs approximately CFA 50,000 to 65,000 and takes up to three month. The required documents are a passport, a commercial card or informal sector card, a financial balance, proof of tax payments, a work contract of at least one employee, CNSS certification, the lease agreement for the business localities and proof of a bank account.

to their degree of formalization, the number of employees that had enrolled with the CNSS, their orderliness in tax and CNSS payments and their orderliness in accounting. In addition, RCPB promised a reduction in the interest rate as financial incentives to clients that registered their activities.

Two issues arise with the implementation of the interventions. First, regarding workshop participation, we cannot distinguish between treatment group clients that participated and treatment group clients that did not participate. Second, while the interest rate reduction and the competition were announced, we are not aware about their implementation. Both issues lead us to interpret our estimates as Intention-to-Treat effects.

5.3 Data and Evaluation Strategy

5.3.1 Data Collection

We use data from three successive rounds of client surveys. The baseline survey was conducted in April 2010, the first follow-up in May 2011 and the second follow-up in November 2011. RCPB had identified a total of 300 clients from three RCPB branches to participate in the intervention and 300 clients from three control branches to serve as the control group. All branches are located in the capital city of Ouagadougou. In our analysis we include only those clients that we observe at all three surveys, i.e. 243 individuals from control and 248 individuals from treatment branches. Table 5.1 shows the number of individuals per branch. We observe at most 90 individuals per branch.

5.3.2 Summary Statistics

The implementing partner, RCPB, selected the participating branches, conducted the allocation to treatment and control and, within branches, selected the partici-

Table 5.1: Number of observations

Branch			
Treatment Branches			
Cissin	81		
Dapoya	90		
Sig-Noghin	77		
Control Branches			
Dassasgho		77	
Gounghin		84	
Song Taaba		82	
Total	248	243	491

pating clients. It is likely that the selected branches were targeted for their specific characteristics, e.g. ease of logistical implementation. On the individual level the sample selection followed a non-random strategy. Only informal clients were selected for whom formalization is a valuable alternative from RCPBs point of view. Also RCPB stated that clients who have problems operating informally will be prioritized. The targeting on the branch and individual level certainly has implications for the generalizability, i.e. the external validity, of our results.

The internal validity - the validity of inferences about whether a relationship is causal - might be affected by the selective targeting if treatment and control group have structurally different characteristics (Todd, 2008). Table 5.2 presents summary statistics of treatment and control clients at baseline. It includes the number of observations for both groups, the mean of the respective variable, the difference in means, and the p-value from a t-test for statistical significance of the mean difference.

It shows that some client characteristics significantly differ between treatment and control before the start of the intervention. The share of female clients is at 18 percent in the treatment group and about 10 percent higher in the control group. Age of the client differs significantly but the disparity is relatively low. The size of

the household shows no significant difference. In both groups clients have on average almost nine household members of which two earn income.

Table 5.2: Summary Statistics by Treatment Group

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-diff	p-value
Female client	247	243	.18	.27	-.09	.02
Age of client	238	242	40.79	42.17	-1.37	.08
Household (HH) size	246	243	8.58	8.68	-.1	.83
Number HH income earners	247	243	1.96	2.1	-.14	.34
Children in school	234	223	.91	.99	-.07	.00
Client's education						
No formal schooling	248	243	.37	.27	.1	.02
Primary	248	243	.38	.26	.12	.00
Secondary	248	243	.3	.3	.00	.96
Higher Secondary	248	243	.04	.06	-.02	.26
Informal apprenticeship	248	243	.09	.13	-.04	.17
Technical formation	248	243	.08	.09	-.01	.7
Age of business	237	237	15.23	13.95	1.28	.09
Sector of activity						
Commerce	243	237	.89	.81	.08	.01
Services	243	237	.04	.11	-.07	.00
Production	243	237	.05	.04	.00	.87
Agriculture	243	237	.00	.01	.00	.55

Continued on next page...

... table 5.2 continued

Variable Names	Sample Size Treated	Sample Size Control	Mean Treated	Mean Control	Mean -difference	t-test p-value
Construction	243	237	.02	.03	-.01	.53

While the average age of the business is around 15 years in both groups we see some considerable differences in the sector of activity. Commercial activities make the bulk in both groups but are significantly more prevalent in the treatment sample with 89 percent compared to 81 in the control group. The control group, on the other hand has significantly higher engagement in service activities. Agriculture or construction are very rare in both groups. For the client's education we also see considerable differences.

The differences in observables lead us to assume that selective targeting took place in some way when RCPB implemented the pilot interventions and selected the clients that form now the treatment group. This gives rise to assume that unobservable characteristics suffer equally from selection bias. We discuss in the next session how our evaluation design mitigates this bias. Another potential source of bias lies in sample attrition. After data cleaning, roughly 17 percent of both treatment and control group are lost due to incomplete information. We cannot provide evidence for the underlying reasons of this attrition.

5.3.3 Evaluation Design

To answer the question whether RCPBs clients in the treatment branches showed increased rates of formalization we apply a difference-in-difference identification strat-

egy. Since the data was collected before and after the intervention we can deal in this fashion with preexisting time constant differences between the treatment and the control group. If not addressed properly these differences would potentially introduce bias in our impact estimates.

Time inconstant differences might equally occur and pose a threat to the assumption of parallel time trends that underlies the difference-in-difference identification. To mitigate this second source of bias matching strategies have been proposed (Ravallion, 2008). Through propensity score matching we focus our analysis on control and treatment observations that have a similar probability at baseline to be allocated for treatment. In other words, based on the observable characteristics, the two groups look alike. This increases the likelihood of them having similar time-trends.

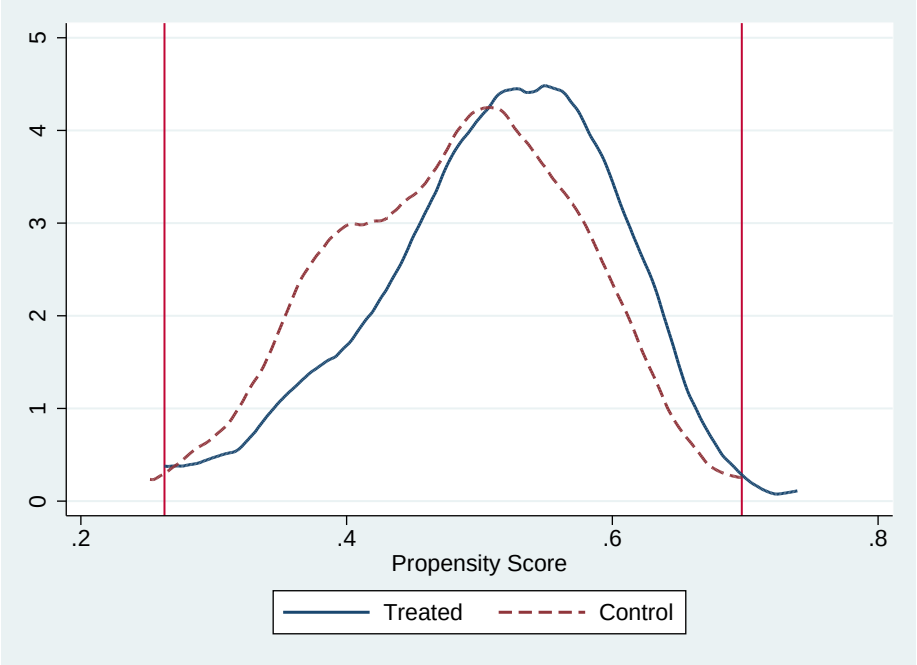

Figure 5.1: Distribution of Propensity Score at Baseline

5.3.4 Estimation Strategy

To mitigate the problem of targeted program selection we estimate the propensity score at baseline. This index allows us to verify how similar the two groups are in terms of propensity to be treated, based on a set of observable characteristics[7][8]. Figure 5.1 shows the distribution of the propensity score for both, the treatment and the control group. The kernel density estimates are cut off at the respective outer boarders. The vertical lines designate the region of common support (.26, .74). We delete 20 observations located outside of these bounds, i.e. 7 controls at the lower and 13 treatment clients at the upper end, under the rationale that these observations have no corresponding match in the respective other group.

On the basis of this *matched* sample we estimate the following equation for individual i to obtain the difference-in-difference estimator DD[9]:

$$Y_{it} = \alpha + DD(D_i I(t > 0)) + \beta D_i + \sum_{p=1}^{2} \gamma_p t_p + \delta X_{it} + \epsilon_{it} \tag{5.1}$$

where Y_{it} is the outcome variable of interest, t_p are dummy variables for period $p = 1, 2$, D_i indicates affiliation to a treatment branch and X_{it} contains control variables assessed at baseline. By interacting the treatment indicator D_i with a dummy variable that takes value one for all post-intervention periods we obtain an estimate of the overall impact. To assess the period specific impacts at $t = 1$ and $t = 2$ we estimate the following equation:

[7] We include the covariates gender of the client, number of household members, dummies for different types of education and the age of the enterprise and a dummy taking value one if the client's enterprises generated above 1 Mio FCFA at baseline. The education variables are all dummy variables, each taking on value one for primary education, for secondary education or more, for an informal apprenticeship or for a formal technical formation, respectively.

[8] See Todd (2008) for a detailed description of the propensity score.

[9] See chapter IV for a discussion of difference-in-difference estimation.

$$Y_{it} = \alpha + \sum_{q=1}^{2} DD^q(D_i t_p) + \beta D_i + \sum_{p=1}^{2} \gamma_p t_p + \epsilon_i \tag{5.2}$$

where DD^1 gives us an estimate of the impact at follow-up I and DD^2 at follow-up II.

In all estimations we apply heteroskedasticity-robust standard errors. One might suspect that the intraclass correlation coefficient is large within branches. This would generally call for inference based on clustered standard errors. However, due to the low number of clusters in our analysis this kind of correction might lead us to underestimate the intraclass correlation (Angrist and Pischke, 2008).

5.4 Results

This section presents the empirical results of RCPB's formalization pilot. Generally, the tables that present estimation results contain difference-in-difference coefficient estimates for the impact of the intervention and the according p-values. The estimates reported in Columns (1) and (2) are based on the estimation of Equation 5.1 and measure the intervention's impact by the difference in change between baseline and all post-baseline observations. We refer to this as the overall treatment effect. The estimation results in Column (1) are obtained without inclusion of covariates. For the estimation results in Column (2) control variables are included. In Columns (3) and (4) we present the period specific impacts obtained by estimation of Equation 5.2. We include the same control variables that were used for estimation of the propensity score.

5.4.1 The Impact on Formalization

Formalization Status

As pointed out in section 5.2 different degrees of formalization exist and entrepreneurs can register with two institutions, the MEBF, where entrepreneurs register to fulfill their tax liability, and CEFOR that issues commercial and informal sector cards. At baseline, the availability of informal sector cards is low. The share of clients who hold informal sector cards increases from 11 percent to 16 percent in the treatment group. The control group has a similar share at baseline, but at follow-up II we only observe a share of 4 percent of informal sector cards. The difference at follow-up II is statistically significant, what indicates that registrations as informal entrepreneur were successfully fostered by the intervention.

The share of clients that hold commercial cards is already substantial at the outset in both groups, but especially so in the treatment group. At baseline 41 percent of the treatment clients have a commercial card, compared to 34 percent in the control group. The mean difference is statistically significant at the 10 percent level, giving some indication for the endogenous program placement. At follow-up I 51 percent of the treatment group hold a commercial card. However, the share decreases again at follow-up II, where only 35 percent of the treatment group state to have a commercial card. This provides further evidence that RCPBs intervention was successful in fostering registrations in the short term, but the effect could not be sustained, as one year after the intervention registration rates drop below the initial level.

Table 5.3: Summary Statistics on Formalization Status by Treatment Group

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-diff	p-value
No card						
Baseline	248	243	.46	.48	-.02	.63
FS I	248	243	.3	.34	-.04	.35
FS II	248	243	.27	.26	.01	.86
Informal sector card						
Baseline	248	243	.11	.09	.02	.51
FS I	248	243	.13	.12	.01	.75
FS II	248	243	.16	.04	.12	.00
Commercial card						
Baseline	248	243	.41	.34	.07	.09
FS I	248	243	.51	.42	.09	.05
FS II	248	243	.35	.36	-.01	.87
Fiscal attestation						
Baseline	248	243	.09	.04	.05	.02
FS I	248	243	.17	.09	.08	.01
FS II	248	243	.1	.11	-.01	.71
Social security						
Baseline	248	243	.05	.06	-.01	.8
FS I	248	243	.13	.08	.05	.09

Continued on next page...

... table 5.3 continued

Variable Names	Sample Size Treated	Sample Size Control	Mean Treated	Mean Control	Mean -difference	t-test p-value
FS II	248	243	.09	.07	.02	.44

The share of clients that have a UFI number is also unbalanced at baseline. In the treatment 10 percent fulfill this formalization requirement compared to only 4 percent in the control group. This difference is significant at the 5 percent level. While the share in the control group increases, the share in target group shows more fluctuation over the period of investigation. Regardless, both groups end up with roughly 10 percent of clients having a UFI number at follow-up II. While this represents a 100 percent increase for the control group, the treatment group is at its initial level. Inscription with the CNSS start out with 5 percent in the target and 6 percent in the control group. In the target group this share goes up to 13 percent at follow-up I and then slightly decreases again. The control group on the other hand does not see large fluctuation at all.

Table 5.4: Summary Statistics on Registration status by Treatment Group

Variable Names	Sample Size Treated	Sample Size Control	Mean Treated	Mean Control	Mean -diff	t-test p-value
Not registered						

Continued on next page...

... table 5.4 continued

Variable Names	Sample Size Treated	Sample Size Control	Mean Treated	Mean Control	Mean -difference	t-test p-value
Baseline	242	242	.42	.4	.02	.71
FS I	248	238	.32	.35	-.03	.48
FS II	192	183	.3	.4	-.1	.05
Registered as informal						
Baseline	242	242	.4	.42	-.02	.71
FS I	248	238	.5	.52	-.01	.78
FS II	192	183	.54	.45	.09	.09
Registered as formal						
Baseline	242	242	.18	.18	.00	1
FS I	248	238	.18	.13	.04	.19
FS II	192	183	.16	.15	.01	.81
Pays TVA						
Baseline	248	243	.2	.17	.03	.35
FS I	248	243	.25	.17	.08	.03
FS II	248	243	.17	.16	.00	.89
Pays industrial fee (BIC)						
Baseline	248	243	.15	.17	-.02	.64
FS I	248	243	.22	.1	.12	.00
FS II	248	243	.15	.06	.09	.00
Pays patent fees						
Baseline	248	243	.88	.77	.11	.00

Continued on next page...

... table 5.4 continued

Variable Names	Sample Size Treated	Sample Size Control	Mean Treated	Mean Control	Mean -difference	t-test p-value
FS I	248	243	.88	.8	.08	.02
FS II	248	243	.67	.68	-.01	.89

Regarding the registration status with MEBF both groups start with around 40 percent of clients who are not registered. At follow-up I this share decreases in both groups. In the treatment group it remains low at follow-up II while it increases to the initial level in the control group. The difference in unregistered clients is statistically significant at endline. In the target group informal registrations increase by 14 percent from 40 to 54 percent. In the control group informal registration increase in the first period but decrease afterwards to 42 percent which is below the initial level. The share of formal registrations shows a small decline from 18 to 16 percent in the treatment group. In the control group it decreases by 3 percent from 18 to 15 percent.

Table 5.5 presents the difference-in-difference estimation results of the impact on formal recognition expressed by possession of official documents, i.e. informal sector and commercial card. We observe a positive and significant impact on the share of individuals holding informal sector cards. The overall effect is small and insignificant at around 5 percent. However, the estimate for follow-up II amounts to a significant 10 percent increase. Respecting the underlying assumptions this gives prove for a causal effect of the intervention on informal sector card possessions. Inspection of Figure 5.2 also shows that the share of RCPB clients holding such a card continuously

increased in the treatment branches while on the other hand in the control branches a sharp decline after follow-up I is observed. A similar pattern occurs for registration as informal business in Figure 5.4. The share for both control and treatment group clients that registered their informal activity is at around 40 percent at the outset, increases until follow-up I, continuous to increase in the treatment group but levels off in the control group.

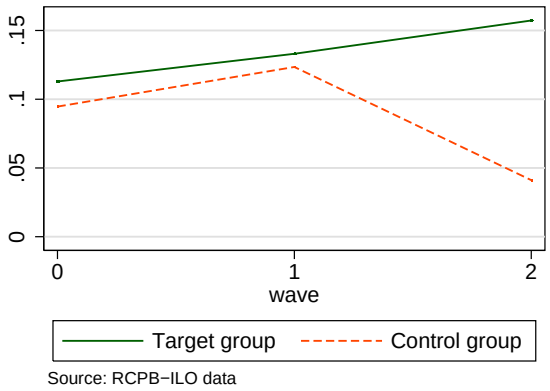

Figure 5.2: Informal sector card

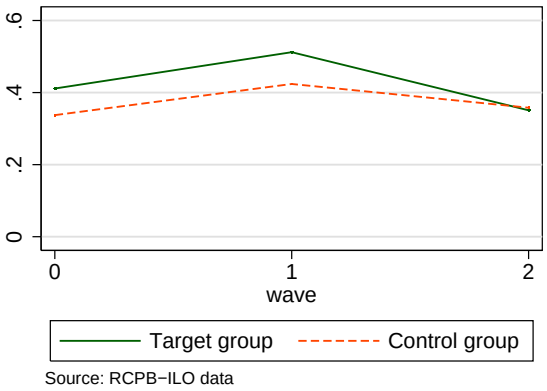

Figure 5.3: Commercial card

Table 5.5 presents the difference-in-difference estimation results of the impact on card possession. We observe a positive and significant impact on the share of individuals holding informal sector cards. The overall effect is small and insignificant at around 5 percent. However, the estimate for follow-up II amounts to a significant 10 percent increase. Respecting the underlying assumptions, this gives proof for a causal effect of the intervention on informal sector card possessions. Figure 5.2 shows that the share of clients holding such an informal sector card continuously increased in the treatment branches while it decreased in the control branches after follow-up I.

We observe that the share of clients who state that they have a UFI number is affected negatively. The estimate at follow-up I is low and insignificant but positive.

This reverses at follow-up II where we obtain a negative impact estimate ranging from 7.2 to 8 percent with significance at the 10 percent level. This suggests that clients cancel their registration with CEFOR in the long run.

Table 5.5: Impact on Formalization

Outcome	(1)	(2)	(3)	(4)
No card				
Overall treatment effect	-.003	-.004		
	.96	.94		
Treatment effect FS I			-.03	-.03
			.63	.63
Treatment effect FS II			.025	.029
			.69	.66
Informal sector card				
Overall treatment effect	.038	.046		
	.27	.2		
Treatment effect FS I			-.013	-.012
			.76	.76
Treatment effect FS II			.089	.122
			.02	.01
Commercial card				
Overall treatment effect	-.049	-.051		
	.38	.37		
Treatment effect FS I			.00	.00

Continued on next page...

... table 5.5 continued

Outcome	(1)	(2)	(3)	(4)
			1	.99
Treatment effect FS II			-.098	-.116
			.12	.09
Fiscal card				
Overall treatment effect	-.025	-.022		
	.42	.49		
Treatment effect FS I			.021	.022
			.58	.57
Treatment effect FS II			-.072	-.08
			.05	.06
Social security card				
Overall treatment effect	.034	.037		
	.23	.22		
Treatment effect FS I			.051	.051
			.15	.14
Treatment effect FS II			.017	.017
			.6	.64
Not registered				
Overall treatment effect	-.074	-.069		
	.19	.21		
Treatment effect FS I			-.054	-.049
			.39	.42

Continued on next page...

... table 5.5 continued

Outcome	(1)	(2)	(3)	(4)
Treatment effect FS II			-.1	-.095
			.14	.15
Registered as informal				
Overall treatment effect	.044	.039		
	.45	.49		
Treatment effect FS I			.007	.001
			.91	.98
Treatment effect FS II			.091	.089
			.19	.2
Registered as formal				
Overall treatment effect	.03	.03		
	.49	.48		
Treatment effect FS I			.047	.048
			.34	.32
Treatment effect FS II			.009	.007
			.86	.9

Note: This table reports regression coefficients and p-values.

The inference is based on heteroskedasticity-robust standard errors.

The regressions use all observations from all waves (pooled sample).

In the regressions reported in (2) and (4) we include individual controls.

Despite the insignificant impact estimates, Figure 5.4 shows that the share of informal registrations increased. From baseline to follow-up I the trend of informal registrations fares quite similar for the treatment and the control group. After follow-up I, treatment group clients continue to register their activities as informal, contrary to the control group where we observe a decline. Again, the assumption of identical time trends suggests that the treatment group would have suffered from the same decline in the absence of the intervention. Figure 5.5 depicts the situation for formal registrations. In the treatment group the share of formally registered clients is constant between baseline and follow-up I and shows even a slight decrease at follow-up II.

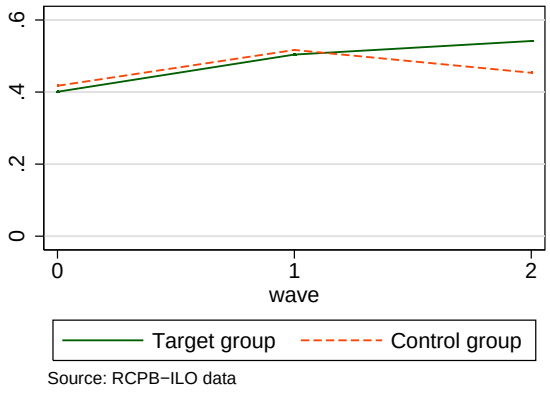

Figure 5.4: Registered as informal

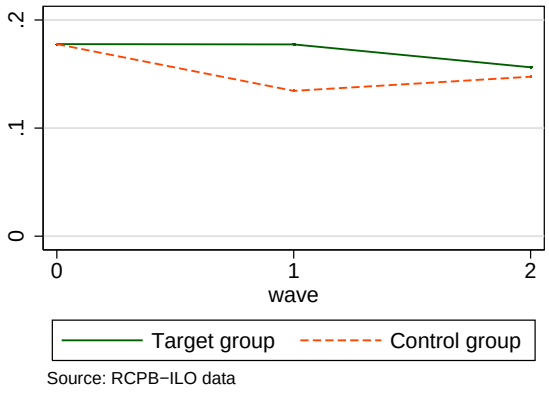

Figure 5.5: Registered as formal

Our results weakly point to the conclusion that a shift from unregistered activities to informal sector activities occurred as a result of the intervention. The estimates for the latter suggest a significant 10 percent increase. This is supported by a 10 percent increase in informal registrations, despite the estimates being insignificant for informal registrations. However, no impact can be documented with regard to formal registrations. The overall effect and the period specific effects are all small and insignificant. Further, the wave specific estimates indicate that the availability of

informal sector cards only increased in the second period. This might result from long processing periods of the authorities. Two prerequisites for formalization, holdership of a fiscal and a commercial card increase first, but then drop again at the last survey round. Unsatisfactory experience with a higher degree of formality is one natural explanation for this.

Tax Payments

A major factor in the formalization decision is the payment of taxes and other fees to the authorities. From the client surveys we observe two sorts of payments that formalized entrepreneurs have to make. A value-added-tax that is levied on services and goods, *Taxe valeur ajouté* (TVA) and a tax levied on commercial and industrial benefits, *Bénéfice industriel et commercial* (BIC). Our descriptive evidence for TVA payments shows a temporary increase in the treatment group, followed by a decline. This pattern is identical to that of commercial card possession and slightly similar to the pattern we observe for formal registrations (compare with Figures 5.3 and 5.5). This gives additional evidence for the explanation that formalization was incited, but not sustained. The overall estimations are all insignificant, but the coefficient signs confirm our explanation. The overall effect is close to zero, the impact in the first period is positive, and the impact in the last period is negative.

For BIC payments we observe as well the triangle shaped pattern in the treatment group. A modest increase of ten percent in the first period, is succeeded by a decline of the same magnitude. Here, however, the estimated coefficients are positive and highly significant. As we see, in Figure 5.7 it is the decline in BIC payments in the control group that drives these estimation results.

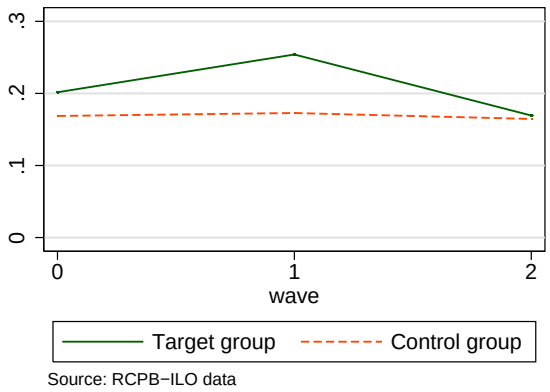

Figure 5.6: Paying TVA

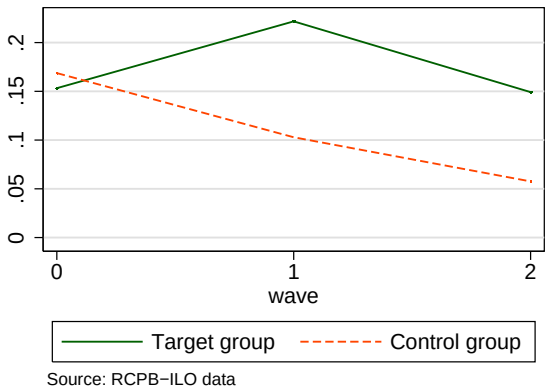

Figure 5.7: Paying BIC

Table 5.6: Impact on Payment of Taxes

Outcome	(1)	(2)	(3)	(4)
Pays TVA				
Overall treatment effect	.006	.005		
	.88	.91		
Treatment effect FS I			.047	.042
			.37	.41
Treatment effect FS II			-.034	-.043
			.49	.43
Pays BIC				
Overall treatment effect	.106	.117		
	.01	.00		
Treatment effect FS I			.123	.123
			.01	.01
Treatment effect FS II			.089	.11

Continued on next page...

... table 5.6 continued

Outcome	(1)	(2)	(3)	(4)
			.04	.02
Pays patent fees				
Overall treatment effect	-.079	-.082		
	.08	.05		
Treatment effect FS I			-.03	-.034
			.54	.48
Treatment effect FS II			-.128	-.144
			.02	.00

Note: This table reports regression coefficients and p-values.

The inference is based on heteroskedasticity-robust standard errors.

The regressions use all observations from all waves (pooled sample).

In the regressions reported in (2) and (4) we include individual controls.

Membership in Professional Organizations

We inquire whether the clients are members in professional organizations and ask for three different memberships: MEBF, sector associations and other business association. We observe that in all three types of professional associations membership decreases for the treatment group. Figure 5.8 and 5.9 display the graphs for MEBF and other memberships. The impact estimates in Table 5.7 are in line with this. We obtain negative and significant estimates of the overall impact and the period

specific impact on membership in the MEBF. The impact on membership in other professional associations is also negative. It remains a puzzle why these effects occur.

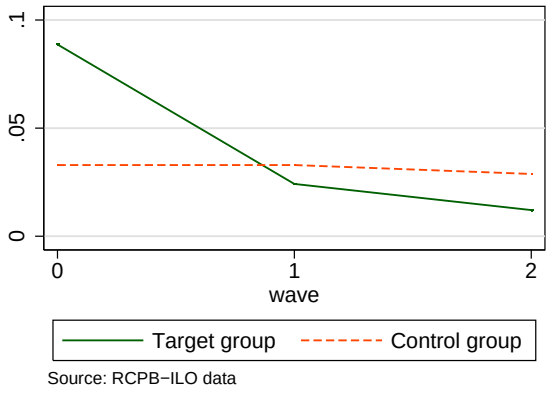

Figure 5.8: Member in *Maison de l'Entreprise*

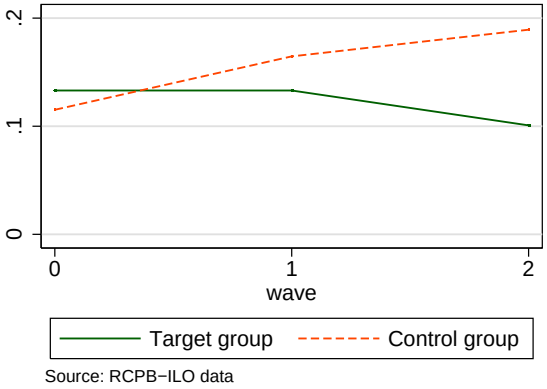

Figure 5.9: Member in other business associations

Figure 5.8 reveals that MEBF membership is constant in the control group. Since no change happened here our difference-in-difference coefficients are equal to a before and after comparison. A possible explanation of the stark decline in MEBF membership after the baseline might be that some treatment group clients interpreted their relation to the MEBF as a membership and at the awareness workshop understood that they have a different status. Membership in other associations also decreased in the treatment group but not as dramatically. Here, it is also the increase in the control group that drives the significance of the negative impact estimate.

Access to Financial Products

In Figure 5.10, we see that the treatment group has a higher share of entrepreneurs who have already a bank account at baseline. The difference amounts to 10 percent and is statistically significant (Table 5.8). Also the share of clients holding separate accounts for private and business purposes is not balanced but here it is the control

Table 5.7: Impact on Membership in Professional Associations

Outcome	(1)	(2)	(3)	(4)
Maison de L'entreprise				
Overall treatment effect	-.057	-.06		
	.01	.01		
Treatment effect FS I			-.055	-.055
			.03	.03
Treatment effect FS II			-.06	-.066
			.02	.02
Sector association				
Overall treatment effect	-.072	-.081		
	.06	.04		
Treatment effect FS I			-.042	-.042
			.35	.35
Treatment effect FS II			-.102	-.132
			.02	.01
Not in any association				
Overall treatment effect	-.007	-.012		
	.76	.6		
Treatment effect FS I			.004	.004
			.7	.71
Treatment effect FS II			-.019	-.034
			.68	.5

Note: This table reports regression coefficients and p-values.
The inference is based on heteroskedasticity-robust standard errors.
The regressions use all observations from all waves (pooled sample).
In the regressions reported in (2) and (4) we include individual controls.

group that has a higher share at baseline (Figure 5.11). Both outcomes increase in the treatment group.

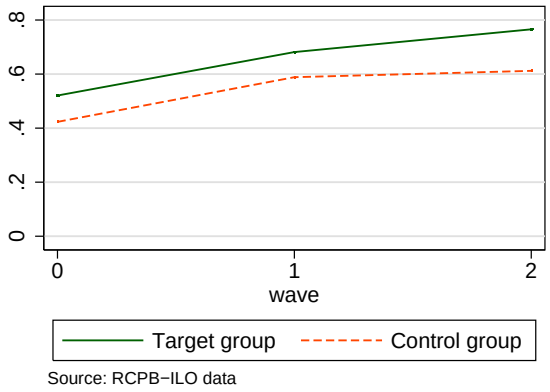

Figure 5.10: Business bank account

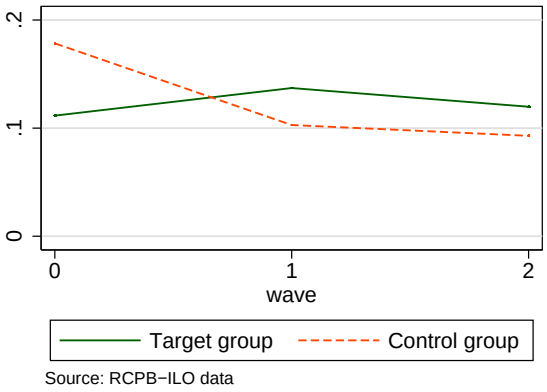

Figure 5.11: Separate bank accounts

Table 5.8: Summary Statistics on Access to Financial Products by Treatment Group

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-diff	p-value
Business bank account only						
Baseline	242	241	.52	.42	.1	.03
FS I	248	243	.68	.59	.09	.03
FS II	192	183	.77	.61	.15	.00
Private account only						
Baseline	242	241	.02	.00	.02	.06
FS I	248	243	.04	.03	.01	.64
FS II	192	183	.03	.03	.00	.82
Same account						

Continued on next page...

... table 5.8 continued

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
Baseline	242	241	.34	.39	-.05	.24
FS I	248	243	.15	.28	-.13	.00
FS II	192	183	.08	.27	-.18	.00
Seperated accounts						
Baseline	242	241	.11	.18	-.07	.04
FS I	248	243	.14	.1	.03	.24
FS II	192	183	.12	.09	.03	.4
Personal insurance						
Baseline	248	240	.08	.07	.01	.56
FS I	248	243	.1	.19	-.09	.01
FS II	192	183	.17	.16	.00	.94
CNSS affiliation						
Baseline	248	239	.11	.17	-.06	.06
FS I	248	243	.13	.16	-.04	.21
FS II	192	183	.16	.13	.03	.4
Theft						
Baseline	248	243	.03	.01	.02	.1
FS I	248	243	.03	.02	.02	.26
FS II	248	243	.02	.00	.02	.06
Fire						
Baseline	248	243	.03	.01	.02	.1

Continued on next page...

... table 5.8 continued

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
FS I	248	243	.04	.01	.03	.04
FS II	248	243	.03	.03	.00	.97
Other						
Baseline	248	243	.02	.01	.01	.27
FS I	248	243	.02	.03	.00	.75
FS II	248	243	.00	.01	-.01	.08
Business not insured						
Baseline	248	243	.06	.01	.05	.00
FS I	248	243	.00	.00	.00	.32
FS II	248	243	.26	.29	-.03	.4

The impact on ownership of bank accounts is positive but insignificant (5.9). Yet, we obtain positive and significant estimates for the impact on whether the client holds a separate bank account for private and business issues. Since formalization requires a bank account it can be assumed that the treatment group opened new accounts in the name of the business. The effect size is 10 percent which matches the increase in registrations of informal status.

Table 5.9: Impact on Bank Account Ownership

Outcome	(1)	(2)	(3)	(4)
Business account				
Overall treatment effect	.04	.042		
	.47	.46		
Treatment effect FS I			.008	.009
			.9	.89
Treatment effect FS II			.082	.084
			.22	.21
Private account				
Overall treatment effect	-.019	-.019		
	.24	.24		
Treatment effect FS I			-.017	-.017
			.38	.38
Treatment effect FS II			-.022	-.022
			.29	.29
Same account				
Overall treatment effect	-.117	-.117		
	.03	.03		
Treatment effect FS I			-.093	-.092
			.11	.12
Treatment effect FS II			-.148	-.149
			.01	.01

Continued on next page...

... table 5.9 continued

Outcome	(1)	(2)	(3)	(4)
Seperated accounts				
Overall treatment effect	.096	.095		
	.02	.01		
Treatment effect FS I			.101	.101
			.02	.02
Treatment effect FS II			.088	.087
			.06	.06
Personal insurance				
Overall treatment effect	-.059	-.06		
	.08	.08		
Treatment effect FS I			-.101	-.098
			.01	.01
Treatment effect FS II			-.006	-.01
			.9	.83
Loan from commercial bank				
Overall treatment effect	-.008	-.01		
	.78	.74		
Treatment effect FS I			.00	.00
			1	1
Treatment effect FS II			-.017	-.024
			.59	.5
Loan from other MFI				

Continued on next page...

... table 5.9 continued

Outcome	(1)	(2)	(3)	(4)
Overall treatment effect	.006	.002		
	.73	.93		
Treatment effect FS I			-.009	-.009
			.7	.68
Treatment effect FS II			.021	.015
			.27	.46

Note: This table reports regression coefficients and p-values.

The inference is based on heteroskedasticity-robust standard errors.

The regressions use all observations from all waves (pooled sample).

In the regressions reported in (2) and (4) we include individual controls.

Neither take-up of loans from commercial banks nor take-up of loans from other microfinance institutions were affected significantly. Interestingly, as Table 5.10 shows, loans from other microfinance institutions where significantly more frequent in the control group at baseline and decreased in the treatment group from 2 percent to zero during the period of investigation.

Table 5.10: Summary Statistics of Take-up of Other Loans by Treatment Group

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-diff	p-value
Loan from commercial bank						
Baseline	248	243	.07	.08	-.01	.81
FS I	248	243	.1	.1	0	.94
FS II	248	243	.04	.06	-.02	.38
Loan from MFI						
Baseline	248	243	.02	.05	-.03	.04
FS I	248	243	0	.05	-.05	0
FS II	248	243	0	.02	-.02	.1

Figure 5.12 shows that the prevalence of personal insurance for health, life, etc. increased in both groups. In the control group the increase is steeper until follow-up I, but ultimately both group see an increase from roughly 10 percent to 17 percent overall. The estimates of the impact on personal insurance emphasize the initially stronger increase in the control group. The overall effect is negative and significant despite the fact that both groups have similar shares at baseline and follow-up II. This results from the strong and highly significant effect of 10 percent at follow-up I. The impact at follow-up II is virtually zero.

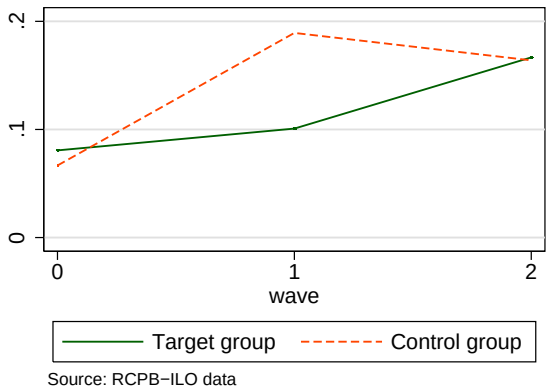

Figure 5.12: Any personal insurance

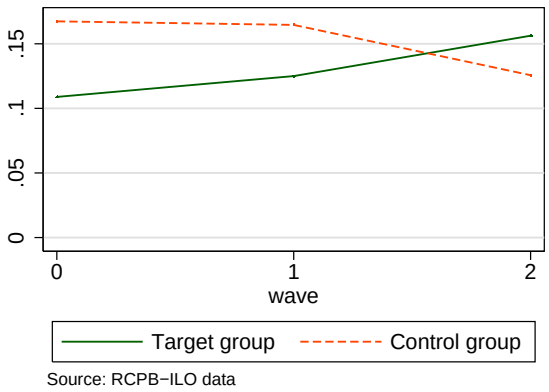

Figure 5.13: CNSS Inscription

Social Security and Business Insurance

In the treatment group we observe an increase in inscriptions with the CNSS (Figure 5.13). While the estimate of the overall impact on CNSS inscriptions is positive but insignificant, the estimate at follow-up II is significant at the 10 percent level and amounts to 9 percent (Table 5.11). Given that we do not observe a similar impact on formal registrations we conclude that it is entrepreneurs that were already formal from the outset that got incited to enroll with CNSS as a result of the intervention.

As Table 5.11 shows, we estimate a small but significant negative impact on the indicator "Business is not insured". In other words, the share of insured enterprises increased. However, none of the impact estimates for business insurance (i.e. theft, fire etc.) is significant. For "Other insurances" we observe a slightly significant negative effect in the last round, but the overall effect is insignificant.

Table 5.11: Impact on Take-up of Business Insurances

Outcome	(1)	(2)	(3)	(4)
Business insurance Theft				

Continued on next page...

... table 5.11 continued

Outcome	(1)	(2)	(3)	(4)
Overall treatment effect	-.002	.00		
	.89	1		
Treatment effect FS I			-.004	-.005
			.83	.81
Treatment effect FS II			.00	.006
			1	.75
Business insurance Fire				
Overall treatment effect	-.006	-.005		
	.7	.77		
Treatment effect FS I			.009	.008
			.65	.67
Treatment effect FS II			-.021	-.022
			.29	.35
Business insurance Other				
Overall treatment effect	-.017	-.018		
	.2	.2		
Treatment effect FS I			-.013	-.013
			.49	.49
Treatment effect FS II			-.021	-.025
			.09	.08
Business not insured				
Overall treatment effect	-.063	-.059		

Continued on next page...

... table 5.11 continued

Outcome	(1)	(2)	(3)	(4)
	.02	.00		
Treatment effect FS I			-.051	-.051
			.00	.00
Treatment effect FS II			-.075	-.069
			.09	.02
Affiliated with CNSS				
Overall treatment effect	.051	.049		
	.2	.2		
Treatment effect FS I			.02	.019
			.66	.66
Treatment effect FS II			.091	.089
			.06	.06

Note: This table reports regression coefficients and p-values.

The inference is based on heteroskedasticity-robust standard errors.

The regressions use all observations from all waves (pooled sample).

In the regressions reported in (2) and (4) we include individual controls.

5.4.2 The Impact on Attitude

Perception of Formality

An array of questions inquires about the clients perception of and attitude towards different issues related to formalization. From these we constructed dummy variables. The innovation clients are more aware of issues related to formalization

now. The impact estimates on attitude are presented in Table 5.12. The first item assesses whether the clients think there is not enough information available to consider formalization. The impact on this item is positive at the first follow up negative at the second and overall close to zero. For the opinion that the process of formalization takes to long we estimate positive effects that are almost significant at the ten percent level.

The impact on the statement that the process of registration is too expensive is negative and significant especially in the first period. It can be assumed that this effect is driven by entrepreneurs that registered at follow-up I since the effect coincides with the wave where we observe an impact on registrations as informal enterprises. The effect on the statement that the process takes too long is positive in the first period, but not significant.

Though throughout insignificant, the impact on the item stating that the formalization process is too complicated is close to zero in the first period and negative in the second.

Awareness about tax liability increased significantly. The period specific effects are above 10 percent and highly significant especially in the second period. Regarding the statement that tax payments are complicated we cannot document any conclusive impact. Regarding problems with the inspectors we observe an effect close to zero in the first wave and a slightly larger positive effect in the second wave. Both are insignificant but the increase in effect size over time might suggest that the clients made some negative experience.

The effects on the attitude item that registration reduces the flexibility for hiring and firing of employees also suggest a change in attitude. At the first follow-up this item shows a significant negative effect of 13 percent. At follow-up II the sign

changed and treatment clients are now rather induced to agree with the statement. Though the 10 percent effect is not significant.

For the last three statements in Table 5.12 we observe large effects, i.e. the intervention caused clients to disagree with the statements that formalization obliges entrepreneurs to pay social security contribution for employees and to adhere to working and security standards. In a sense, these effects prove that the treatment clients equate formalization with registration to the semi-formal status.

Table 5.12: Impact on Attitude Towards Formalization

Outcome	(1)	(2)	(3)	(4)
There is not sufficient information available to consider of the activity				
Overall treatment effect	-.002	.004		
	.97	.95		
Treatment effect FS I			.026	.033
			.69	.62
Treatment effect FS II			-.036	-.032
			.62	.66
The process of registration takes too much time				
Overall treatment effect	.06	.059		
	.28	.28		
Treatment effect FS I			.102	.102
			.1	.1
Treatment effect FS II			.009	.008

Continued on next page...

... table 5.12 continued

Outcome	(1)	(2)	(3)	(4)
			.89	.9
Too expensive (the cost of registration is too high)				
Overall treatment effect	-.132	-.134		
	.02	.02		
Treatment effect FS I			-.159	-.164
			.01	.01
Treatment effect FS II			-.098	-.098
			.16	.16
Once the activity is registrated one needes to fill out and submit numerous documents which is to complicated				
Overall treatment effect	-.017	-.016		
	.78	.79		
Treatment effect FS I			.013	.013
			.84	.84
Treatment effect FS II			-.053	-.051
			.45	.47
Once the activity is registrated one needs to pay taxes				
Overall treatment effect	.134	.136		
	.02	.02		
Treatment effect FS I			.117	.119
			.09	.08
Treatment effect FS II			.155	.156

Continued on next page...

... table 5.12 continued

Outcome	(1)	(2)	(3)	(4)
			.02	.02
It's not a problem to pay taxes, but the system is so complicated that it is better not to register				
Overall treatment effect	.008	.009		
	.89	.88		
Treatment effect FS I			.053	.052
			.43	.43
Treatment effect FS II			-.046	-.044
			.51	.53
Inspectors come and harass the entrepreneurs				
Overall treatment effect	.027	.03		
	.65	.62		
Treatment effect FS I			.003	.004
			.97	.95
Treatment effect FS II			.056	.06
			.43	.39
Once registered, there is less flexibility to employ and discharge employees				
Overall treatment effect	-.03	-.029		
	.61	.62		
Treatment effect FS I			-.134	-.132
			.04	.05

Continued on next page...

... table 5.12 continued

Outcome	(1)	(2)	(3)	(4)
Treatment effect FS II			.096	.095
			.17	.18
Once registered, one needs to pay social security contributions for the employees				
Overall treatment effect	-.059	-.057		
	.32	.33		
Treatment effect FS I			-.211	-.207
			.00	.00
Treatment effect FS II			.124	.123
			.07	.07
Once registered, one needs to respect working standards				
Overall treatment effect	-.115	-.114		
	.05	.05		
Treatment effect FS I			-.228	-.226
			.00	.00
Treatment effect FS II			.022	.021
			.75	.76
Once registered, one needs to respect security standards				
Overall treatment effect	-.114	-.112		
	.06	.06		
Treatment effect FS I			-.217	-.213
			.00	.00

Continued on next page...

... table 5.12 continued

Outcome	(1)	(2)	(3)	(4)
Treatment effect FS II			.012	.011
			.86	.87

Note: This table reports regression coefficients and p-values.

The inference is based on heteroskedasticity-robust standard errors.

The regressions use all observations from all waves (pooled sample).

In the regressions reported in (2) and (4) we include individual controls.

Perception of Informality

We obtain large significant effects on the clients' perception of informality. The impact estimates indicate that the share of clients who believe informality is an obstacle to training and other support services decreases significantly by 44 percent. The share of clients who agree with the statement "I cannot bid for advertised requests for qualifications" decreases by roughly 10 percent. When asked about difficulties in accessing financial services the impact estimates indicate a highly significant 20 percent decrease in the share of clients who suffer from such problems.

These results suggest, that the treatment clients assess the conditions of operating in the informal sector more positively after participating in the intervention. When we link this to the low sustainability of registrations, one could assume that the favorable perception of conditions in the informal sector are driven by negative experience with higher degrees of formality.

The intervention has not had a significant effect on the question whether client spend a lot on health costs for employees, neither on the questions whether maternity costs or bribe payments to the authorities need to be paid. On the question whether inexplicable losses occur through employees we observe a strong positive impact, yet we fail to understand why the intervention could have caused this.

Table 5.13: Impact on Perception of Informality

Outcome	(1)	(2)	(3)	(4)
I cannot directly respond to advertised requests for qualifications				
Overall treatment effect	-.088	-.088		
	.11	.1		
Treatment effect FS I			-.107	-.107
			.08	.08
Treatment effect FS II			-.064	-.064
			.32	.32
My access to financial services is more difficult				
Overall treatment effect	-.222	-.223		
	.00	.00		
Treatment effect FS I			-.192	-.193
			.00	.00
Treatment effect FS II			-.258	-.26
			.00	.00
My access to training services and support for the development of my activity is more difficult				

Continued on next page...

... table 5.13 continued

Outcome	(1)	(2)	(3)	(4)
Overall treatment effect	-.438	-.439		
	.00	.00		
Treatment effect FS I			-.465	-.467
			.00	.00
Treatment effect FS II			-.405	-.405
			.00	.00
I have to pay a lot to cover my employees costs of sickness when they are sick				
Overall treatment effect	.019	.016		
	.75	.78		
Treatment effect FS I			.069	.064
			.29	.33
Treatment effect FS II			-.044	-.043
			.52	.54
I need to pay when my employees go on maternity leave.				
Overall treatment effect	.012	.009		
	.82	.87		
Treatment effect FS I			-.026	-.032
			.66	.59
Treatment effect FS II			.06	.06
			.37	.37
I have to bribe the puble authorities representatives				

Continued on next page...

... table 5.13 continued

Outcome	(1)	(2)	(3)	(4)
Overall treatment effect	.069	.067		
	.21	.22		
Treatment effect FS I			.082	.079
			.19	.2
Treatment effect FS II			.053	.052
			.43	.43
I suffer easily from inexplicable losses through my employees				
Overall treatment effect	.314	.313		
	.00	.00		
Treatment effect FS I			.249	.248
			.00	.00
Treatment effect FS II			.395	.395
			.00	.00

Note: This table reports regression coefficients and p-values.

The inference is based on heteroskedasticity-robust standard errors.

The regressions use all observations from all waves (pooled sample).

In the regressions reported in (2) and (4) we include individual controls.

5.4.3 The Impact on the Client's Business Turnover

Our main outcome indicator with respect to the clients' business is monthly turnover. We observe whether turnover of the clients' business in the last month

lies within three brackets: Below 500,000 FCFA, between 500,000 and 1,000,000 FCFA or above 1,000,000 FCFA. Again the descriptives indicate a bias in treatment allocation towards larger firms. Table 5.15 shows that 56 percent among treatment and 48 percent among control clients report turnover in the turnover bracket above 1 Mio FCFA. The difference is significant at the 10 percent level and potentially threatens internal validity of the results. We observe that a share of roughly 30 percent of the clients has turnover in the lowest bracket and 20 percent in the middle bracket. Figures 5.14 - 5.16 display how turnover in the two groups evolves over the

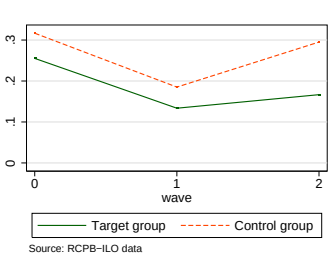

Figure 5.14: Turnover $\leq$ 500,000

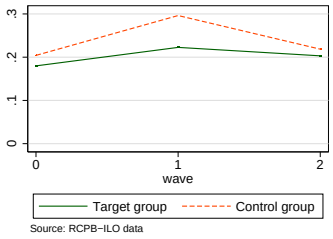

Figure 5.15: Turnover 500,000 $-$ 1,000,000

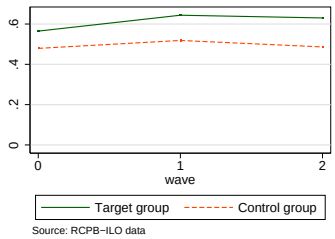

Figure 5.16: Turnover $\geq$ 1,000,000

course of the project. Apart from the differences in levels, the pattern is identical for treated and controls. Also the estimation results presented in Table 5.14 provide no evidence for an impact of the intervention[10].

Table 5.14: Impact on Turnover

Outcome	(1)	(2)	(3)	(4)
$\leq$500,000 FCFA				
Overall treatment effect	-.027	-.023		

Continued on next page...

[10]Important to mention is that the data on turnover is incomplete for follow-up II; roughly 20-25 percent of the sample have missing values.

... table 5.14 continued

Outcome	(1)	(2)	(3)	(4)
	.59	.64		
Treatment effect FS I			.007	.012
			.9	.81
Treatment effect FS II			-.071	-.069
			.24	.25
500,000-1,000,000 FCFA				
Overall treatment effect	-.027	-.029		
	.56	.53		
Treatment effect FS I			-.051	-.055
			.35	.31
Treatment effect FS II			.003	.005
			.95	.92
≥1,000,000 FCFA				
Overall treatment effect	.054	.052		
	.35	.36		
Treatment effect FS I			.044	.043
			.5	.5
Treatment effect FS II			.068	.064
			.33	.36
Returns covers exp totally				
Overall treatment effect	.051	.046		
	.35	.4		

Continued on next page...

... table 5.14 continued

Outcome	(1)	(2)	(3)	(4)
Treatment effect FS I			.048	.041
			.43	.5
Treatment effect FS II			.053	.052
			.42	.43
Returns cover exp partially				
Overall treatment effect	-.047	-.042		
	.37	.42		
Treatment effect FS I			-.045	-.037
			.45	.53
Treatment effect FS II			-.051	-.049
			.44	.44
Returns do not cov exp				
Overall treatment effect	-.01	-.01		
	.59	.6		
Treatment effect FS I			-.008	-.008
			.7	.71
Treatment effect FS II			-.012	-.012
			.54	.56

Note: This table reports regression coefficients and p-values.

The inference is based on heteroskedasticity-robust standard errors.

The regressions use all observations from all waves (pooled sample).

In the regressions reported in (2) and (4) we include individual controls.

The questionnaire also offers information on whether the businesses' gains are sufficient to cover the expenses. Over 60 percent of the respondents in both groups state that the returns cover expenses fully, for roughly 30 percent returns cover expenses at least partially and only 3 percent report that their returns do not cover the expenses. We cannot observe any significant impact on these indicators and also the impact estimates are insignificant and small in size.

Table 5.15: Summary Statistics of Business Income by Treatment Group

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-diff	p-value
$500,000 FCFA$						
Baseline	239	240	.26	.32	-.06	.14
FS I	247	243	.13	.19	-.05	.12
FS II	192	183	.17	.3	-.13	.00
500,000-1,000,000 FCFA						
Baseline	239	240	.18	.2	-.02	.5
FS I	247	243	.22	.3	-.07	.06
FS II	192	183	.2	.22	-.02	.71
$1,000,000 FCFA$						
Baseline	239	240	.56	.48	.09	.06
FS I	247	243	.64	.52	.13	.00
FS II	192	183	.63	.49	.14	.00

Continued on next page...

... table 5.15 continued

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
Returns cover expenses fully						
Baseline	246	241	.63	.67	-.04	.38
FS I	248	243	.72	.72	.00	.97
FS II	192	183	.68	.67	.01	.83
Returns cover expenses partially						
Baseline	246	241	.34	.29	.05	.27
FS I	248	243	.26	.25	.01	.86
FS II	192	183	.31	.31	.00	.98
Returns do not cover expenses						
Baseline	246	241	.03	.03	.00	.76
FS I	248	243	.02	.03	-.01	.34
FS II	192	183	.01	.02	-.02	.16

We cannot observe any impact on the intervention on business turnover. This is not surprising as, in the short run, one would assume that formalization and its direct costs reduce the profitability but do not affect business turnover.

Management Practices

A major part of the intervention focused on management practices to prepare the clients for formalization (see section 5.2). On this account, we observe considerable improvement. Figure 5.17 shows how the share of clients who keep accounts develops

over time. Control and treatment clients have nearly identical shares in this variable at baseline. At follow-up I, a 35 percent increase occurs in the treatment group. Since this is a self-reported measure we need to interpret it cautiously as treated clients might be inclined to give desirable responses. Interestingly, in the treatment the share decreases slightly afterwards and at follow-up II both groups have similar levels again.

Table 5.16 informs on the kind of book-keeping that the clients apply. We see that a specific cash-book where earnings and expenses are noted is the most frequent practice in both groups. The share of clients that report to keep records in that way is 10 percent higher in the control group at baseline but doubles from 30 to 60 percent in the treatment group. Also the practice of keeping a stock inventory becomes more frequent over the time of investigation.

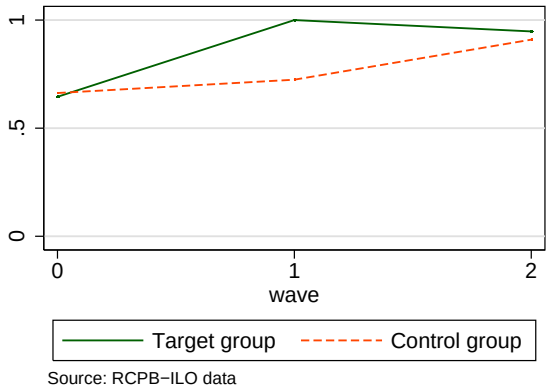

Figure 5.17: Any book-keeping

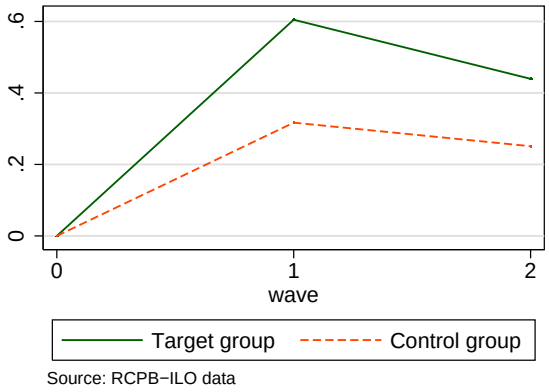

Figure 5.18: System of management was improved since last interview

Figure 5.18 presents the shares of clients who state that they have implemented improvements in their management practices. Again, the large difference in favor for the treatment group needs to be viewed with reservation as it is self-reported. Table 5.16 also lists whether these improvements have been beneficial to the clients' busi-

ness. Unsurprisingly, close to no entrepreneur reports to have implemented useless changes.

Table 5.16: Summary Statistics on Book-keeping Practice by Treatment Group

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-diff	p-value
Any book keeping?						
Baseline	248	243	.65	.66	-.02	.69
FS I	248	243	1	.72	.28	.00
FS II	248	243	.95	.91	.04	.1
Specific book						
Baseline	245	243	.32	.41	-.09	.04
FS I	248	243	.6	.45	.16	.00
FS II	248	243	.48	.43	.05	.25
Yearly balance						
Baseline	245	243	.02	.01	.00	.71
FS I	248	243	.09	.13	-.03	.22
FS II	248	243	.09	.16	-.07	.02
Stock inventory						
Baseline	245	243	.1	.07	.04	.15
FS I	248	243	.33	.34	-.01	.8
FS II	248	243	.00	.00	.00	
Occasional book-keeper						

Continued on next page...

... table 5.16 continued

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
Baseline	245	243	.15	.09	.05	.08
FS I	248	243	.12	.06	.06	.03
FS II	248	243	.08	.28	-.2	.00
Regular book-keeper						
Baseline	245	243	.05	.08	-.03	.26
FS I	248	243	.07	.09	-.02	.38
FS II	248	243	.08	.07	.01	.54
Changed book-keeping						
Baseline	248	243	.00	.00	.00	
FS I	248	243	.6	.32	.29	.00
FS II	248	243	.44	.25	.19	.00
Benefitted from changes						
Baseline						
FS I	148	76	.97	.89	.08	.01
FS II	108	61	.99	.98	.01	.68

The difference-in-difference estimations confirm the descriptive picture. Book-keeping as well as management improvements have both been affected positively. We estimate an overall effect of 17 to 20 percent on whether book-keeping is applied or not. The effect is highly significant. The overall effect on improvements in management is highly significant as well and ranges from 24 to 27 percent.

Table 5.17: Impact on Book-keeping and Management Practice

Outcome	(1)	(2)	(3)	(4)
Any book keeping?				
Overall treatment effect	.184	.201		
	.00	.00		
Treatment effect FS I			.298	.295
			.00	.00
Treatment effect FS II			.069	.078
			.17	.14
Improved management				
Overall treatment effect	.233	.261		
	.00	.00		
Treatment effect FS I			.278	.279
			.00	.00
Treatment effect FS II			.188	.239
			.00	.00

Note: This table reports regression coefficients and p-values.

The inference is based on heteroskedasticity-robust standard errors.

The regressions use all observations from all waves (pooled sample).

In the regressions reported in (2) and (4) we include individual controls.

Employment

In Figure 5.19 we see that the share of clients who have employees is high with 91 percent in the treatment and 85 percent in the control group. While the trends look very similar, the estimates in Table 5.19 suggest a negative impact on employment. The share of clients who have any employees decreases overall by 6 percent according to the estimation with covariates. This effect is significant at the 10 percent level only. The wave specific impact amounts to 8.6 percent and is significant at the 5 percent level. On the intensive margin we do not obtain significant results but the estimate for the impact on formal employees is positive while the the number of informal employees was reduced. Given the additional cost of employment that a formalized

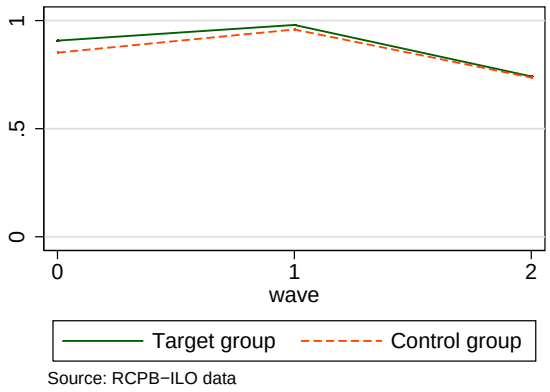

Figure 5.19: Any Employees

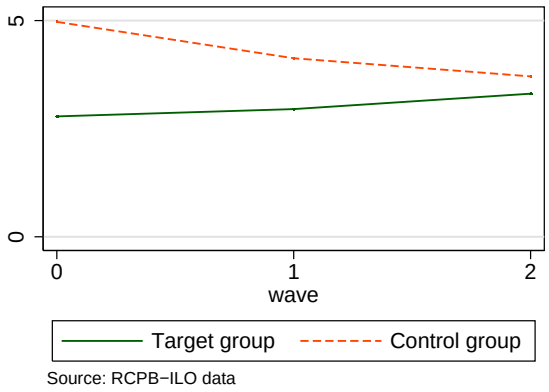

Figure 5.20: No. of Formal Employees

enterprise faces one would hypothesize a negative impact of the intervention on employment for enterprises with few employees. Therefore, it comes as no surprise that we see a decrease in the share of clients who have any employees. Also, the opposing directions of the impact on formal and informal employees can be explained as formalized switch from having informal employees to employing formally.

Table 5.18: Summary Statistics on Employment by Treatment Group

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-diff	p-value
Any employees						
Baseline	248	243	.91	.85	.06	.06
FS I	248	243	.98	.96	.02	.18
FS II	248	243	.74	.74	.01	.89
Number of formal employees						
Baseline	51	67	2.78	4.97	-2.19	.1
FS I	64	62	2.95	4.13	-1.18	.07
FS II	42	31	3.31	3.71	-.4	.69
Number of informal employees						
Baseline	207	181	4.92	4.81	.11	.86
FS I	225	208	4.36	4.73	-.37	.49
FS II	170	172	4.44	4.1	.34	.5
Number of employees with social security						
Baseline	38	44	2.05	2.34	-.29	.41
FS I	50	39	2.52	3.87	-1.35	.06
FS II						
Any new inscriptions with social security						
Baseline						
FS I	205	241	.09	.09	.00	.96

Continued on next page...

... table 5.18 continued

	Sample Size		Mean		Mean	t-test
Variable Names	Treated	Control	Treated	Control	-difference	p-value
FS II	192	182	.02	.02	.00	.94
Number of social security inscriptions						
Baseline						
FS I	20	22	1.65	3.09	-1.44	.06
FS II	4	4	3.25	1.5	1.75	.26

Table 5.19: Impact on Employment in Clients' Business

Outcome	(1)	(2)	(3)	(4)
Any employees				
Overall treatment effect	-.053	-.059		
	.15	.06		
Treatment effect FS I			-.038	-.037
			.26	.26
Treatment effect FS II			-.069	-.086
			.17	.01
Number of formal employees				
Overall treatment effect	1.395	1.247		
	.3	.33		
Treatment effect FS I			1.07	.861

Continued on next page...

... table 5.19 continued

Outcome	(1)	(2)	(3)	(4)
			.44	.49
Treatment effect FS II			1.953	1.932
			.23	.26
Number of informal employees				
Overall treatment effect	-.47	-.525		
	.51	.45		
Treatment effect FS I			-.685	-.721
			.39	.36
Treatment effect FS II			-.195	-.277
			.8	.72

Note: This table reports regression coefficients and p-values.

The inference is based on heteroskedasticity-robust standard errors.

The regressions use all observations from all waves (pooled sample).

In the regressions reported in (2) and (4) we include individual controls.

5.5 Conclusion

In this paper we assess the efforts of a large microfinance organization in Burkina Faso to foster changes in the registration status of its clients' enterprises. Our analysis shows that clients venture into formality in the short run, but do not take the final step to become formal. These results persist even after eradication of time-constant differences in the treatment and control group that were caused by targeted program

placement. This indicates that formalization is not perceived a valid option by the entrepreneurs. This fuels the argument that support for microenterprises, e.g. better access to credit and social protection, should not be conditional on their registration status (Sparks and Barnett, 2010).

Our analysis fails to provide insight to the reasons why higher rates of formalization were not achieved. It can be assumed that from the entrepreneurs' perspective the benefits of formalization did not outweigh the costs. Gelb et al. (2009) study the informal sector in East and South Africa and conclude that registrations of informal firms remain rare in a weak business environment characterized by low quality of infrastructure and lack of financial services. Consequently, they propose that formalization is only a viable option if its costs are reduced. In Burkina, such efforts are being implemented, as the rate of taxes that semi-formalized enterprises pay is lower than in the formal sector. Still it seems that the costs of formalization are too high, or that other parameters oppose permanent formalization.

Gërxhani (2004) reviews the literature on the informal sector and concludes that the belief predominates that the disadvantages of a large informal sector outweigh the advantages. With this in mind, it should be further determined what it is that kept RCPBs clients from formalizing. Identifying these issues can enable policymakers with an interest in supporting micro- and small-scale entrepreneurial activities to create a more favorable business environment.

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Curriculum Vitae

Education

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